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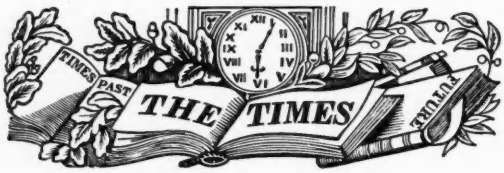

READING WITHOUT TEARS

WHEN Mr. Kipps's solicitor saw the prospectus of the Associated Booksellers' Trading Union, Limited, he at once suspected fraud. It was far too well printed, Mr. Bean thought, for a reputable undertaking.

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DISCOVERY

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Science in a Modern World

THE British Association met this August in Cambridge, where a great part of the world's science has been carried on. Hundreds of visitors listened to accounts of new discoveries in the actual rooms where Rutherford, Clerk Maxwell and J. J. Thomson worked from day to day. They saw one of the prisms which were used by Isaac Newton, the greatest scientist of all and the man who between eighteen and thirty thought more concentratedly on one subject than perhaps any human being who has ever lived. They went to a garden party in the College of Charles Darwin.

At the meetings themselves, science has never shown itself more alive. The new theory of the nucleus, discovered by a great Danish scientist, Niels Bohr, as explained in his own words (this was reported by Dr Infeld in *Discovery* for August): the fascinating results on the six hormones which are just appearing; Blackett's account of the cosmic rays and the recently discovered particle called the "yukon" or heavy electron; there were so many new things that no one heard them all, and yet everyone felt a reflected sparkle through being in the middle of one of the great periods of achievement.

And yet, as visitors walked through the College gardens, reading newspapers to find out exactly what they had missed the day before, they could not help reading also of other achievements, this time of applied science. For while they had been listening to the wisdom of Niels Bohr, high explosive was being tossed about in Spain and China; and during the week-end which Blackett began by explaining how cosmic rays may originate in the supernovae, heavy guns were on their way towards the Rhine. Ordinary decent people everywhere were thinking "What is going to become of our world?" If you have travelled this summer, you must

have heard those words in Berlin as well as in Prague, in the Midi, in Paris and London. And you will have heard once or twice at least, a bleaker remark: "Ah well, a man can only die once".

In a time like this, scientists would be less than human if they did not consider their responsibilities. For the first time, the meetings of the British Association did not take place as though this particular existence, this pleasant island of discourse in the Cambridge gardens, were going on for ever. Scientists, at any rate as citizens, have an obligation to take their part in these critical years; and during the last few months, and in public at the Cambridge meeting, there was much hard thought about the positive relations of science to the world.

Science and War

Lord Rayleigh and Professor Southwell (in a brilliant speech published in this number of *Discovery*) both dealt with this problem in their presidential addresses. The position of most scientists can be summarized:

1. Science itself is a non-moral activity. It aims only at understanding the natural world; and, in its applied form, it gives us power over the natural world. Thus the results of biochemical research, if no economic considerations were involved, would enable us to lengthen the average life of Englishmen by at least 10%; but they would also enable us to halve the population tomorrow by judicious poisoning. Whether either or none of these applications is carried out, depends on society and not on science.

2. Science can no more prevent itself producing poison and high explosives than a cure for diabetes, or the steam engine. It can no more prevent itself making possible the bombing aeroplane than the ordinary motor-car.

It is admitted that much research is directed consciously and entirely towards producing means of destruction. Whether an individual scientist takes part in such work is a problem for his own conscience; but in fact any discussion of this topic is quite unrealistic, for in every country in the world there are enough competent scientists who, for one reason and another, would be willing to undertake such work. Incidentally, military inventions do not require Rutherfords and Bohrs; second-rate technicians are quite adequate.

"War", Leonardo wrote coolly in his diary, "is a bestial madness" and went on designing siege engines for Cesare Borgia. It is no use pretending that science will not continue to produce the means of war; or that individual scientists will not feel themselves justified in doing whatever society requires.

3. The scientific means of war have, however, been grossly exaggerated; large-scale killing is one of the most inefficient of technical achievements. The next war will be horrible enough, but not of a different order of horror from the last. 10,000,000 people were killed between 1914 and 1918; it would be difficult to

double the number. Air raids have gripped the imagination of many, but the experience of Barcelona, and even of Canton, shows that it is hysteria to talk of wiping out a large town.

The most drastic effect of science on warfare is not large-scale killing, but large-scale organization. The ability to control millions of men and transfer them from place to place. Given modern organizations upon millions of men, there would be almost as many killed if warfare were conducted only by sword and bows and arrows.

4. Any utopian solution, in which it is hoped that science or scientists will not co-operate in war can be rejected as a dream. But that does not eliminate the responsibility of scientists in trying to prevent war—as members of the community and specifically as members of one of the most influential professional groups in the world.

Science and Society

In fact, point 4 above has been accepted whole-heartedly by many scientists. Living in a world of crisis, they have been compelled to learn that war is a symptom of society's sickness, not a single phenomenon on its own account. To understand the causes of war, in order to prevent it, we must understand society itself: in particular we must understand the change in the world since applied science enabled the whole scale of industry and organization to change within a hundred and fifty years. Until that change is understood, and until those powers of applied science are directed consciously to the benefit of the people of the world, human life will be a precarious business at best.

Few men have realized the urgency of this problem so clearly as the present editor of *Nature*, Sir Richard Gregory. Within the scientific profession he has also commanded the greatest respect and affection: and, to a very large extent because of his personal devotion, he has led other scientists to a realization as vivid as his own. His gospel has been: We must *know* as much of the social relations of science as we do of science itself. Since the industrial revolution, science has been changing the world; we must understand exactly how the change is working if we are to keep our hope for the human race. And we must understand as coolly and as dispassionately as scientists aspire to think in their own fields.

Gregory has now persuaded organized science to follow him. At Cambridge, the British Association did an unprecedented thing: it formed a special "division" for the study of the Social Relations of Science, The word "division", adopted for the first time for this special purpose, was intended to suggest a high degree of autonomy for the S.R.S. It will have most of the freedom of a new society within the framework of the only general scientific institution in the country. By next August it will have taken some special instance of the impact of science on society, investigated it as many-sidedly as possible, and presented its

first report. This part of the year's work may be important when some louder-voiced actors have been irretrievably forgotten.

Science and Education

Whatever the S.R.S. does, it has started to try to find out the truth, which is curiously unfashionable in a civilization whose standards are slipping backwards. We are none of us, of course, immune from the grosser imbecilities of our time; step by step we are drifting into a belief that propaganda, or organized lying, to use a true phrase for once, is a reputable addition to the affairs of mankind. The surprising thing among human beings to-day is that, considering they are never allowed to hear an impartial word, they remain so tolerant and calm. More than half the children in Europe in 1938 are being taught deliberate lies every day of their school life: if any one doubts it, he should read the history books used in European schools to-day. The majority of children in Europe stand no chance whatever of looking at the world with clear eyes. It is not a performance of which any adult person alive ought to be proud.

And it behoves us all the more to try to find the truth and teach the truth ourselves. If we are tempted to resort to propaganda, that is, counter-lies, then we lose too much. S.R.S. must tell the truth; and in our schools we must do the same, so long as our privileged position endures.

H. G. Wells has hammered at his message of education all his life; it *does* matter what you teach, the facts of the world are important as well as the way you think about them. There is no excuse for false information, and there is as little for not giving information at all. At Cambridge, Wells was making this evident again, with his incomparable force and initiative. Sometimes he has underestimated his own influence; in fact, he has passed so much into our minds that we do not always recognize the debt.

As he has said so long, scientific education must take a much greater place in schools than it now does; even now, forty years after Wells was first crusading, Biology is at least as good a training for mental gymnastics as any language, quite apart from its value as *true information*; and of true information we cannot have enough. It is absurd that *The Science of Life* is not given to children in schools as one of their essential and obvious text-books.

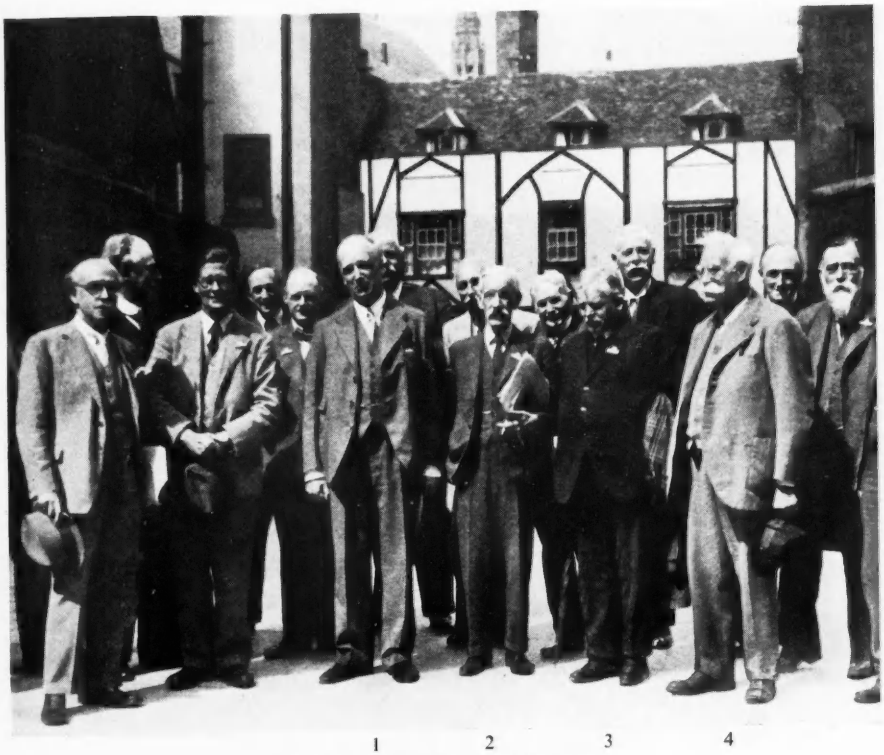
The same is true of physics, though it is a little harder to teach.

C. G. Darwin's presidential address to Section A which is included in this issue ought to be taken to heart by every science master.

Education in the modern world must go hand in hand with science. Whatever happens to us, our fate in the next fifty years depends upon how far society can adjust itself to the new power science has put into its hands. And our culture will be a very mandarin affair unless it includes a knowledge of the most important single force in human affairs.

People were talking and thinking on those lines at the British Association this year. For many, it was the first time that such matters had impinged on them; like most scientists, they were not politically minded, as all they asked was to do their work in peace. But even that small request, they know by now, might not be granted them. So it devolved upon them, as decent citizens, to do what they could. They know that science represents the most precious thing that man has reached to; the right of the free human mind to look for the truth. And a good many thought "Use that right in education. Use it on society and on science's own applications. It may help to bring us through. If not, then we have failed for the time being—but one day men will respect those who used the right, not those who were so frightened that they dare not let other people think."

THE EDITOR



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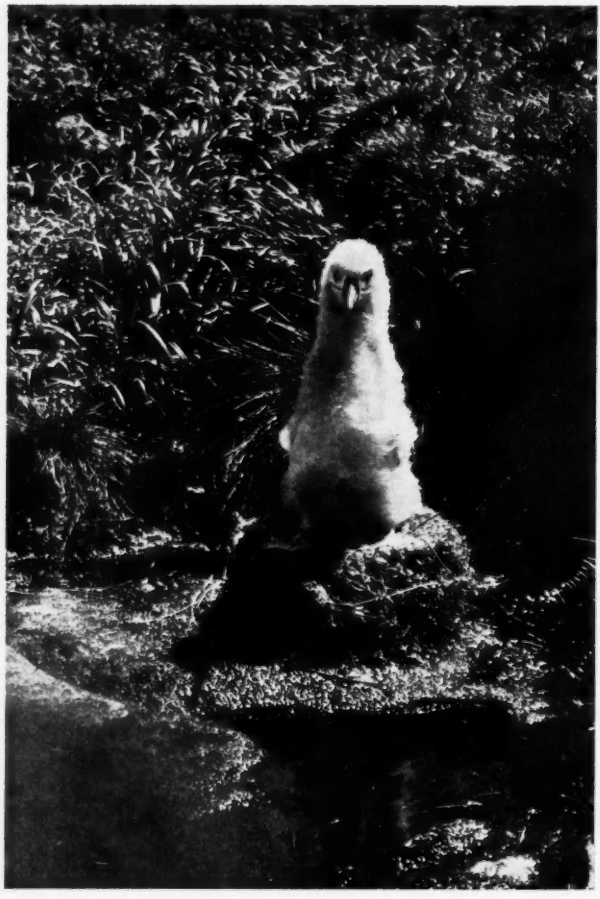
The British Association at Cambridge
An informal group including

1, *Lord Rayleigh.* 2, *Sir F. Gowland Hopkins.* 3, *Professor W. W. Watts.* 4, *Professor Sir Edward Poulton.*



Young molly-mawk on its mud throne

The Norwegian Scientific Expedition to Tristan da Cunha

*

By

ERLING CHRISTOPHERSEN

THOSE lonely monuments of the restless South Atlantic, the one a stupendous pyramid towering about 7500 ft. above the troubled waters imprisoning it, the other a huge fortress-like block with lofty steeples on its southern face, the third simulating a colossal prehistoric animal with a magnificent head and a humped back, represent what is probably the most isolated rock mass on our planet.

The largest, the main island of Tristan da Cunha, is a typical eroded volcanic cone, having a roughly circular outline. Most of it rises vertically from the sea, and these cliffs, 1000–2000 ft. high, incline to a plateau, which stretches inland for one or two miles and then slopes steeply to a crater peak in which there is a small lake.

Miniature crater cones bulge out from this huge one, like clinging parasites, and Stony Hill, the rugged mass of loosely packed blocks of lava with little or no vegetation on the south side, is the result of a final terrific explosion of such a secondary cone scattering the original structure out of recognition. Lineally arranged on the south and north cliffs are deep craters, which have been formed by the dual process of explosion and subsequent subsidence. Filled with small lakes they form one of the most striking and pleasing features of the islands.

By the action of water of heavy rainfalls the once comparatively smooth surface has been dissected into deep ravines, which give, from the distance, a beautiful

Inaccessible Island is appropriately named

vein-like impression. On the peak slopes the ravines are so closely spaced that they are only separated by a sharp knifelike-edge often no more than a few inches wide—a fascinating path for the reckless mountaineer. The encroaching sea is responsible for the formation of the vertical cliffs where the force of gravity has caused millions of tons of solid rock to crash down to the grinding mill of powerful waves.

Situated on the north and north-west side of the island is an old lava plain about half a mile wide and several miles long at about 100–300 ft. above sea-level. At the north corner of this plain a strong stream of beautiful clear water gushes out from beneath the cliff, making this the natural place for a settlement.

Inaccessible Island

The two other islands in the group, Inaccessible and Nightingale, are considerably smaller and lower than Tristan itself, but in many ways even more interesting. They are uninhabited.

By generous permission of His Majesty's Government we have been able to spend almost four months studying nature on these isolated islands. Four wonderful months in contact with a grand and luxuriant nature and the most lovable group of people that we have ever met. Our expedition, the Norwegian Scientific Expedition to Tristan da Cunha under the patronage of H.R.H. the Crown Prince of Norway, was mainly supported by scientific funds, and the Antarctic explorer and whaleship owner Lars Christensen. The party consisted of thirteen men.

We knew beforehand that there are twenty rainy days in each month of the Tristan summer, and that landing usually has to be undertaken in stormy weather. We were not disappointed. A strong northerly wind stirred up a heavy sea when we were approaching the island on the cargo steamer *Anatolia* from Cape Town. The warm wind from the tropics always causes heavy fog where it meets with the cold sub-antarctic air, so that nothing could be seen of the island until we were two or three miles off the settlement. Then suddenly a dark shadow appeared on the horizon, we saw the foaming breakers on the shore, then Herald Point, and after a while the humble cottages of Edinburgh became visible through the mist.

We were at last at our journey's end, with more than two months of travelling behind us.

Three times we blew the whistle, followed by immediate results. Three boats came pulling out for all they were worth, disappearing behind the huge waves or dancing on their crests, and soon they were alongside. I shall never forget our first meeting with these people we had come so far to see. Some were fair, some were dark, all of them had friendly faces, and they laughed and smiled and raised their arms to greet us as we hung over the railing. William Repetto, the chief man, came on board. Father Wilde, who was returning with us, shook hands with him and they kissed each other. A short explanation, William gave his orders, and for

Tristan da Cunha

two days these sturdy men rowed continuously between ship and shore in stormy and rainy weather, landing our boxes, barrels, wood, and other heavy equipment. All this they did, without a chance to barter on board, and without a promise of any kind of remuneration. A pinch of tobacco each was their final reward, and they were exceedingly happy.

The landing place on Nightingale is a shelf in the vertical cliff. The sea washes into a blow-hole close by, each wave forcing the water out through a narrow opening, and spraying our things and ourselves at regular intervals like a rotating garden sprinkler. Large caves were cut into the cliff, some of them having been formed at a time when the sea level was higher. Penguins sat looking at us, phlegmatic and apparently indifferent to all our actions, and the warm air was filled with the odour of guano, bringing back to my memory the days on bird islands in the Pacific. Numerous petrels were sitting on the cliff, terns flew over our heads, and swimming on the sea outside were myriads of the greater shearwater, that wide-ranging bird which is found in our home waters in nearly all seasons, its only known nesting grounds being the Tristan group of islands.

Before dark actually hundreds of thousands of these shearwaters came in from the sea, making an almost deafening noise as they flew above us. Gradually they came down into the tussock grass where they hunted for their holes and spent the night in courtship. It was a strange and marvellous experience to walk through the tussock with a flashlight watching this lively night life of birds on Nightingale. About an hour before daybreak the murmuring and crying of the mating birds died down, then everything was quiet, and before the sun appeared above the horizon they were out at sea again.

Tussock grass is not what we usually call grass. Like a green ocean it covers Nightingale Island almost entirely and looks very inviting from a distance, but a closer examination reveals certain very undesirable facts. It usually grows high above the head, frequently 10 ft. above the ground; the stems are like sticks and densely crowded together in tussocks, and the tussocks in turn are also densely

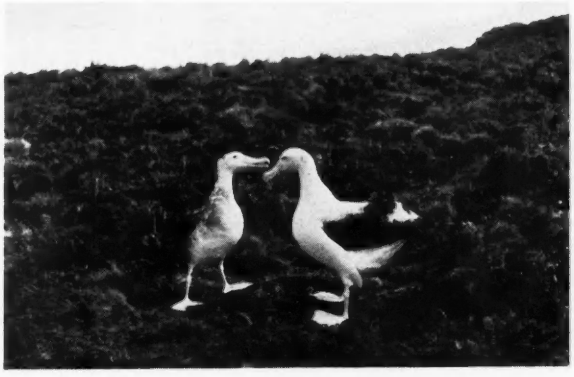
Native Tristan thrushes on tussock grass

crowded, sometimes even completely united into a continuous cover. The leaves are tough and leathery but with razor-like edges. It is no fun trying to force one's way through this scrub. The shortest route between two points is very rarely the quickest. The game is to keep to the high spots on the rock outcrops where it is possible to see in which direction one is travelling. Down below in the tussock ocean it is as well to have a compass. In return for excellent service rendered the birds took good care of the tussock, amply fertilizing the soil so that the grass could grow up high and give them the necessary shelter, for the peculiar Tristan finches and thrushes feed on it, and in it the penguins have their home, while under it the shearwaters dig their burrows.

Every day at breakfast time the penguins came to say good-morning. That is, we had pitched our tents in an open place in the tussock, and it was apparent that these gentlemen in evening dress used to sit here and take a sun bath. Large crowds of them came out of the tussock to see if we had not moved, but as soon as we peeped at them they turned around, fleeing in great disorder. Rock-hopper penguins on the run are a funny sight. All penguins are clumsy on land, but these are undoubtedly among the clumsiest. They jump about with their feet side by side, head and upper part of body stretched in front to keep their balance, which they invariably lose, falling down on their stomachs, while other penguins jump on

Every day at breakfast time—

them and peck at them, until they finally manage to get to their feet again with the aid of their short wings, and try to support themselves, between jumps, on their little tails.



Molly-mawks, a kind of albatross

In the mountains the albatross had its home. Not the large wandering albatross following ships across the ocean, but a smaller species called "molly-mawks" by the Tristanites. The young ones were quite big at this time of the year, sitting one on each nest which consisted of an earthen stool about 1 ft. high. A young molly-mawk showed great courage in defending himself against anyone who dared to come too close to his domain. Already at a distance he opened his long black beak, closing it suddenly with a sharp, clapping sound. This he repeated several times to make sure that there was no mistake. If the visitor, in spite of all, came closer, the clapping was not to be misunderstood, but if he should come still nearer the molly-mawk made a last effort, spewing a red liquid all over the intruder's trousers. At that stage of the game we had to give in, and next time, wise from experience, we made a circle around the nests. If you have ever had to walk about for a whole day with your trousers sprayed with the vilest smelling liquid you can imagine, you will beware never to do it again.

We called them "cap-caps" for their characteristic clapping of beaks, and after a while they won our hearts on account of their heroic self-defence, though it was a sad sight to see them lifted off the throne to be weighed and marked by our ornithologist. At that time of the year the cap-caps were all right. They sat comfortably on their thrones and were fed with octopus and other kinds of excellent sea food, growing fatter every day. After we had gone, however, the story changed. Young molly-mawks are one of the greatest delicacies on Tristan.

The rarest bird on Nightingale is the large finch discovered by Sir Hubert Wilkins, who was naturalist to the *Quest* Expedition. It seems to be dependent on the Phylica trees for its existence, and as there are only a few patches of forest on the entire island its habitat is very restricted. But we found it also on Inaccessible, so that its known range has been considerably enlarged.

Perhaps the most interesting feature of Nightingale is the bogland, not because of the surface of the bogs but because of all the historical information stored in their depths. We dug thousands of years down into them. Oily bird soil and then

layers of moss, roots of trees and stems of tree-ferns were alternated with layers of grass and other plants—silent witnesses of the climatic changes of ages.

A layer of octopus beaks was found in a bog at about 650 ft. above sea-level. Did the sea at one time come up to this level? Indeed a very tempting line of thought, connecting up with theories of melting ice and the filling up of oceans, or with subsidence of land masses. Or should we rather be contented with the fossil beach line observed everywhere in the cliffs above the present level of the sea, thinking that the fishing molly-mawks were particularly numerous at that time, bringing their young cap-caps up on octopus?

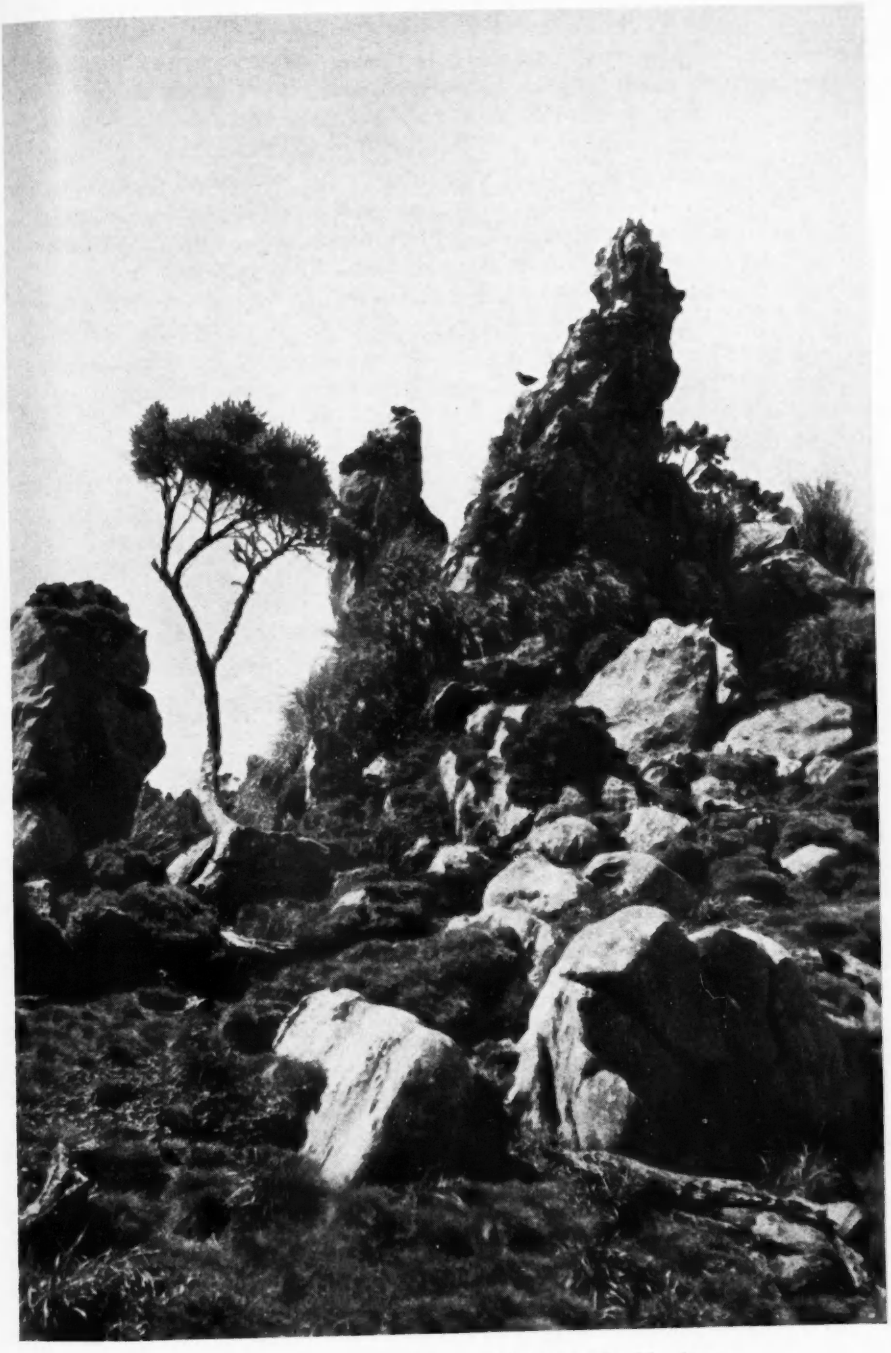
Inaccessible Island is very appropriately named. Not only because it may be very difficult to effect a landing in safety, but because it looks inaccessible, vertical cliffs facing the sea on all sides. Moreover, these cliffs appear to be even more inaccessible and unapproachable because the island is cut off like a truncated cone without a peak. It is quite different from any of the other islands in the group.

We had picked Blendon Hall Point for our landing place, as the ascent to the top plateau is easier here than in any other place. I am afraid, however, that the easiness of the climbing was amply compensated by the difficulty of the landing. It took us two days and the splitting of two boards in our only boat to get ashore, and it took us seven days and the last of our only motor to get away.

One of the world's strangest birds. The island cock of Inaccessible

To choose a few among the great many interesting forms of plant and animal life on Inaccessible is a rather difficult matter. However, that strange little creature, the flightless "island cock" is certainly one of the most fantastic of its inhabitants. The entire island is full of them, they run through the vegetation like mice. When you stop to watch for them they will look up at you with their bright red eyes, but if you stretch your arm out to pick them up they are gone in less than no time. Hagen, our bird man, had to catch them in intricate traps, and we had several specimens in a cage where we could study them more intimately. They did not last long, unfortunately, in spite of the best of care. This is also the experience of the islanders who have tried to keep them in captivity. Inaccessible is 1400 miles from the nearest continent. How this flightless bird ever came here and where it came from is yet an unsolved problem. Perhaps our studies will bring the solution a bit closer.

One of the new plants we found on Inaccessible was a sister of the pepper tree.



Remarkable effects of erosion. A Nightingale Island landscape

The fruit is fleshy and could very well have been distributed by birds, but the only wide-ranging birds in these islands are the sea birds which do not eat fruits. The land birds, likewise, do not appear to be responsible, because none of the four species existing on Inaccessible have been able to carry the plant to Tristan or Nightingale, a negligible distance compared to the vast ocean expanse to the continents. One thing is certain: something must have happened in bygone ages which does not happen any more. To try to lift the veil covering the past history of Tristan is one of the most fascinating problems in front of us.

On our way back to Tristan we paid a visit to the east side of Inaccessible to pick up two of our men who had spent a few days there. At this place, called Salt Beach, an attempt had once been made to start a new settlement. A house had been built and a piece of ground had been cultivated for potatoes, but at the time of our visit it was a sad enough looking place. A piece of the mountain side had come down through the roof of the house, and the potatoes were quite lost in the luxuriant growth of other plants. However, this is of no importance. There is enough land on Tristan itself for those who want to put a stone wall around it and keep it in cultivation.

[*A second report from this expedition, describing and picturing the people of Tristan da Cunha, will appear in a later issue of* Discovery.]

The Work of the Alpine Meteorological Camp on the Summit of the "Rochers de Naye"

(*Among other scientists Professor A. Piccard assisted this expedition in its study of high altitudes, continuing, at the same time, his research into solar radiation and cosmic rays.*)

THE systematic study of the troposphere in all parts of the world is relentlessly pursued, and all air-ports are equipped with instruments enabling observations to be made at great heights. But until now all research had been mainly carried out from the ground, starting from relatively low altitudes. It was therefore interesting to study the condition of the winds in the mountainous regions, where they are subject to the disturbing influence of the irregularities of the earth's surface.

It was with this aim that the Alpine Meteorological Commission—recently created by the Federal Air Office—organized and directed, at the Rochers de Naye (above Montreux, at an altitude of 6500 ft.), an important research establishment where work was carried on from the 10th to the 23rd of August of this year.

Although research was mainly centred on aerial currents and the problem of gliding, many other fields were covered, including agricultural meteorology, scientific methods of combating night-frost in Spring, hydrology, climatology, and National Defence. It is easy to appreciate the possible far-reaching consequences of the research-work carried out at the Rochers de Naye, which may serve as a basis for future studies in the same fields.

Professor Piccard, Professor Jacquerod and Monsieur P. L. Mercanton listening to reports on the research work at Rochers de Naye

About fifty scouts, technicians and students were invited to work for twelve days under the guidance of Monsieur Jacquerod, Professor of Physics at the Neuchâtel University, and President of the Alpine Meteorological Commission. These young men proceeded, several times a day, following a meticulous time-table, to take measurements on the summits of the Rochers de Naye and in the outlying districts; they measured the speed and direction of the wind, the force of aerial currents at high altitudes, the temperature of the air and the earth, of dampness, and of solar radiation.

All these observations, carried out with the utmost precision, called, naturally, for the use of special apparatus in large quantities which it would be impossible to describe in detail here. Wind-gauges, galvanometers, theodolites, sounding-balloons, hydrometers and solar-gauges constituted the bulk of the material brought up to the summit and the neighbouring heights each morning by the scouts and their companions.

Particular use was made of the theodolites—placed at their disposal by the Swiss Central Meteorological Bureau and its Director, Professor P. L. Mercanton—which made possible minute-to-minute recordings of the trajectories of the sounding-balloons and of the gliders.

The gliders were, in fact, of the utmost assistance to the members of the Alpine Meteorology Camp. With the aerodrome of Lausanne-Blécherette as their base they were instructed to fly over the Massif de Naye twice a day, so that they might be followed by the theodolite observation-posts and at the same time co-operate with the ground observers. Thus every morning and afternoon at the same hour a glider was towed from La Blécherette up to a height of 11,500 ft. above the observation-posts. At that altitude the pilot of the glider would release the rope connecting it to the aeroplane, and would continue his flight on his own. He would make it last long enough to make as many observations as possible in

full flight of the conditions of the atmosphere. He would then land at Villeneuve from where he would be taken back to

Lausanne by road in readiness for the next flight. The "towing" aeroplane was made full use of for aerial observations automatically registered on special apparatus on board, measuring the temperature, pressure and dampness of the air.

In addition to this flight-service, directed by Hermann Schreiber, Chief of the Swiss School of Gliding, further air-soundings were carried out twice a day, at a height of nearly 20,000 ft., with the aid of a military 'plane. This machine would leave Lausanne aerodrome, rising to 20,000 ft., passing through successive "floors" of 3500 ft. At this top height, it would fly straight towards the Rochers de Naye. Once above the summit it would start a progressive descent to 6500 ft., describing large circles around the Massif de Naye, and then return to its base at Lausanne.

Finally, in order to carry out observations in still higher atmospheric "planes", the Alpine-Meteorology Camp made use of the advantages of wireless. This was done with the help of small rubber balloons filled with hydrogen which carried small radio transmitters to enormous heights. On a short wave length, these broadcasted information without interruption about the temperature and the pressure and humidity of the air at and above 26,000 ft.

It will be of the greatest interest to compare the observations thus registered with those gathered on board the aeroplanes and gliders.

G. LE CREUSAZ

Logic and Probability in Physics

By C. G. DARWIN

(*After paying a tribute to the late Lord Rutherford, Dr C. G. Darwin continued with the following address to the members of the British Association at Cambridge on August 19th.*)

IN choosing a theme for my address I was in some difficulty. The main subjects of present interest in physics, the nucleus of the atom, cosmic rays, and the phenomena at deep temperatures, are being dealt with in the discussions of our Section, so that they would be excluded even apart from the fact that I cannot speak on them with authority. It would have been possible for me to choose a narrower subject, but I could not feel that this would have possessed the general interest that such an occasion demands, and so with some trepidation I am venturing on an even wider theme and am going to touch on the philosophy of our subject. This is a dangerous thing to do for one who has never made more than the most superficial study of pure philosophy, but still I do not apologize for it, because it appears to me that recent scientific history has revealed a deep schism between the professional philosophers and the scientists, and this schism is worthy of examination.

General philosophy claims to be the critical subject which lays down for all of us what we may be allowed to think, and yet it has played no part whatever in the great revolutions of human thought of the present century—those connected with relativity and the quantum theory. It might have been expected that the scientists would have been constantly consulting the philosophers as to the legitimacy of their various speculations, but nothing of the kind has happened. Since no one can dispense with some sort of metaphysic, each scientist has made one for himself, and no doubt they contain many crudities, but it would seem that a deep interest in metaphysic is a disadvantage rather than an advantage to the physicist—at least I have the impression that those of my friends who are most inclined to speculate on the ultimate things appear to be the ones whose scientific work is most hampered by doing so. Now I propose to risk a similar indiscretion. I want to embody in it the practical philosophy of a physicist, and I do not mean it as an attack on the pure philosophers, who are very reasonable people, only chargeable with the minor offence of not having made me want to read their books!

I had better begin by stating shortly the ideas I intend to discuss. There is a notable contrast between the way we think about things and the way we think we ought to think about them. We have set up as an ideal form of reasoning the formal logic which has held the field since the days of Aristotle. We rarely conform to this ideal, but instead we usually make use of arguments having no accurate axiomatic basis, which compel belief because of some large accumulation of favourable evidence. I intend to develop the idea that the old logic was devised for a world that was thought to have hard outlines, and that, now that the new mechanics has shown that the outlines are not hard, the method of reasoning must be changed. The key to the modification has already long been in our hands in the principle of probability, but whereas in the past constant attempts were made to fit this into the old system, the new mechanics suggests the possibility of a different synthesis. Though I hope this subject will be found interesting in itself, I would not have ventured to bring it forward if I had not also a very practical purpose in doing so, and that is to urge that our mathematical education both at school and university has been gravely deficient in that it has put all the emphasis on matters susceptible of rigorous proof, while it has very completely neglected the equally important subjects of statistics and probability. I shall enter into these matters at the end of my address.

I have said that there is a contrast between the way we all think about things and the way we think we ought to think about them. This is true not merely of the scientist; the layman holds the same belief. I may exemplify this by a quotation from that epitome of the reasoned thought of the ordinary man, the detective story. After Watson has expressed admiration at one of the most brilliant guesses of Sherlock Holmes he is met with the reply: "No, no; I never guess. It is a shocking habit, destructive of the logical faculty." The reader is encouraged to revere the great detective by being told that all his arguments are Aristotelian syllogisms. The

scientist forms his opinions in much the same way as Holmes really did, but he is apt to feel that this is a fault in himself and that he ought to be forming them by the severe principles of formal logic in the manner that appealed to Watson. Of course there are branches of knowledge for which this can be done, but somehow they are not the interesting ones; indeed, outside pure mathematics any subject is apt to become dead and uninteresting as soon as it is brought down to this form. The really live branches of physics call for a very different kind of thought, for a review of a system of interconnected facts and for a perception of conjectured analogies, and so on. This is vaguer, but it is more important, and our system ought to give importance to the important things, so that the actual habit of thought which the intelligent man finds the most useful is acknowledged as the right one.

In general literature there is a particular kind of writing which we all admire on account of its direct simplicity; it is to be found in much English of the seventeenth century, but specially in the work of many French authors, both early and late. It is a delight to read and often admirably achieves its aim of clarifying the subject-matter—but not always. There are two ways of writing simply about a subject. One is to understand and make clear the simplicity of it; the other is to leave out all the difficult parts. When we entertain the idea that everything can be brought down to the Aristotelian syllogism, are we not doing this last? Is it not possible that when a subject is brought down to these terms, it is merely that we have picked out from it the easy parts and concealed all the rest? If we turn our attention to the question of why we believe in our various theories, we can see that there is often a quite illusory simplicity in their presentation.

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WHY do we believe in the various theories that we are all agreed to accept? Once a theory has become well established someone usually gets to work to find a system of axioms, postulates, indefinables and so on from which it may be derived. For example, classical mechanics is based on Newton's Laws, or whatever system has been substituted for them by later criticism. The direct derivation of everything from an axiomatic basis has an attractive simplicity, but it tends to make us think we believe the theory because of the axioms, whereas the axioms are only a convenient shorthand summarizing a wide field of information, and they are believed in merely because we believe in the theory. This may be seen by an occurrence of a few years ago. There was a letter to *Nature* pointing out a rather fundamental contradiction in the quantum theory—I do not think the author meant it as strongly as the accident of his wording implied. One's immediate feeling was that the idea must be wrong (as indeed it proved to be), but the point of my mentioning it is that I found I did not care much whether it was right or wrong, because the quantum theory must be right anyhow. The cumulative weight is so overwhelming that it is not conceivable that anyone could upset it in a single column of *Nature*. A little

doctoring of the axioms would certainly put the matter right again, and hardly anyone would be any the better. We have then to believe that axioms are not important things, but that it is the whole body of accumulated doctrine that matters.

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TAKE it from a different angle. The "logic" school of thought has in its repertory the idea of a "crucial experiment", that is the single experiment which gives the answer yes or no to a whole theory. I suppose the most striking crucial experiment ever done was the Michelson-Morley experiment on æther drift, which was made the basis of the whole gigantic theory of relativity. Michelson and Morley showed that to the order of the square of the earth's velocity there was no æther drift, and they showed it to the limits of the precision of their apparatus. For some twenty years the theory of relativity grew enormously, based on this one experiment, and then it was felt that it would be proper for someone else to repeat the work, and Dr Dayton Miller undertook the task. We cannot see any reason to think that his work should be inferior to Michelson's, as he had at his disposal not only all the experience of Michelson's work, but also the very great technical improvements of the intervening period, but in fact he failed to verify the exact vanishing of the æther drift. What happened? Nobody doubted relativity. There must therefore be some unknown source of error which had upset Miller's work. But as Miller was improving on Michelson, this contains the implication that Michelson's work must have had two unknown sources of error which happened to cancel one another. What has become of the crucial experiment? We do not believe in relativity because of the Michelson-Morley experiment; it is one, and an important one, of a number of cumulative pieces of evidence which all fit together, and it is this cumulation and not any one of its pieces that makes us believe in relativity.

From examples like these we conclude that an axiomatic basis, of the kind demanded for the operations of formal logic, is too narrow for the understanding of the physical world. Something wider is needed. Now for more than a century there has been growing up the recognition that probability plays a part in much reasoning, and that there must exist a wider system of logic which has probability as one of its features. Attempts have been made, and are still being made, to bring probability back into the narrow fold of the old logic. It appears to me that these attempts are hopeless, but before approaching the question directly I want to develop an analogy which seems to me important. Like everyone else I *feel* the compelling power of the old logic, and I cannot *feel* how when we try to go beyond it we can get the same compulsive force. But on the other hand I *know* of a case where our thoughts are driven in one direction by a force which seems to have the same psychological compulsion as does formal logic, and yet where the result is undoubtedly wrong.

To anyone who has thought at all seriously about the world, or at any rate to anyone who has made an elementary study of mechanics, I suppose there is nothing more absolute than the law of causality. By this I mean that the future is completely contained in the present. Passing over obvious examples where this is true, like the path of a projectile or the orbit of a planet, we may take an extreme case where we might expect our faith in the principle would be most severely tried. Take the typical case of chanciness, the tossing of a coin. We know that in a general way there is an even chance of heads or tails, even though we sometimes hear of gifted individuals with muscles so delicately adjusted that they can control the event. But in the ordinary way the tossing of a coin is complicated by being produced by a living organism, so let us simplify the problem by designing a catapult of some kind to project it. Which of us does not believe the coin would fall the same way every time if such a mechanism could be made with really complete precision? When the machine fails to make it do so, we say it is because there may have been a speck of dirt in the lubricant or something like that. In other words, we do not feel that the fall of the coin is determined by chance, but we regard the uncertainty we observe as due to our ignorance of all the detailed causes. Ignorance is a confession of incompetence, and so we regard the existence of chance as a blemish in our otherwise admirable characters. This feeling goes very deep, since we are prevented by it from having the complete control of our surroundings that we somehow think should be our due. We start prejudiced against probability and in favour of causality. So much for what we *feel* about causality; and about thirty years ago this feeling would have been regarded as a piece of inescapable reasoning, with the same kind of compelling power as a logical syllogism. We still have the feeling, but now we know it is wrong, and what is more, we know that it is wrong for a reason we never thought of. To understand this oversight we must review the recent history of atomic theory.

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THIS history of the development of physics in the first quarter of the twentieth century will rank as one of the greatest in the advancement of knowledge, but it will also rank as one of the most curious in the history of human thought. In 1901 Planck started the quantum theory. Even this was curious. He was trying to find out the law of complete radiation by the use of ordinary statistical methods, and observed that he got his answer at what should have been the last stage but one of his work. The last stage would have involved proceeding to a limit, and he found that he got the experimental answer without doing so, and an absurd answer if he did. The work went rather deep into statistical theory and there were many for long afterwards who were not convinced of its compelling force, but it was the great merit of Planck that he knew that he had got something involving a quite revolutionary idea—the quantum. In succeeding years other phenomena were seen to

involve the same revolutionary idea: Einstein's theory of the photo-electric effect, and of the ionization produced by X-rays, this theory of specific heats, later improved by Debye, and Bohr's theory of spectra. All these things fitted in quite obviously with the quantum, but quite as obviously they violently contradicted the physics of the nineteenth century. What should a man think about a beam of light which according to Einstein had to be composed of arrows, whereas a hundred years earlier Fresnel had *proved* that it was a system of waves? What does a rational being do when faced with two mutually contradictory but both indubitable pieces of evidence? It was a nice test for the critical spirit, and it revealed a wide divergence of choices. In making a historical judgment long after the event, one of the hardest things to do is to recall the relative scale of importance which contemporaries were inclined to attach to the different branches of their subject.

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THE statistical theory of matter had already been well established by the work of Maxwell, Boltzmann and Gibbs, but it was not regarded as an essential part of a general mathematical-physical education. For example, in the various courses I was advised to undertake during my undergraduate career, no one at any stage ever suggested to me that I should learn anything about the kinetic theory of gases. I think that that period was one when the Cambridge mathematical school was not at its best, and very probably a little more was done at other places, but, to judge by the available text-books in any language, statistical theory was not regarded as one of the prime subjects of study, as it would be now. The period was essentially dynamic, and as such it was moderately easy for it to take in the new ideas of relativity, to which indeed the experimental work of the last century had been leading. But there was no common habit of thought on statistical lines, and so there was a sharp separation of opinion. The seniors, impressed with the vast mass of successful physics of the nineteenth century, with only a rather general knowledge of statistical theory but no facility of thought in it, found the new ideas completely contrary to their convictions. Such men would think that these ideas depended on the difficult and unfamiliar conceptions of statistics and would be inclined to judge that there must be a fallacy in the statistics which would be cleared up later. On the other hand the laboratory workers, dealing with atoms and electrons from day to day, could not fail to be more impressed with the discontinuous phenomena and the beautiful way these could be explained by the quantum. Such men would cheerfully accept the Bohr orbits as a complete explanation of the hydrogen spectrum, and certainly in many cases would be actually ignorant of the difficulty, the monstrous absurdity, of supposing that a sharp jump from one orbit to another could be responsible for a train of waves shown by the spectroscope to be lasting for quite a long time. So the majority of rational beings behaved in the natural human way of managing to forget all the disagreeable facts. But not every one, for there were Bohr and the other leaders who recognized the difficulties on both

sides but could still maintain an attitude of balance and could believe that from somewhere there would come a higher synthesis by which everything would be fitted together.

As time went on the quantum got obviously stronger and stronger, and began to invade more fields. The nuclear atom in the hands of Bohr showed itself capable of giving all the broad details of the periodic table of chemistry, still with nothing done to meet the awful difficulties of optical theory. But about 1925, guided by the Correspondence Principle, things were moving towards a tentative theory of the refractive index, and it was this that finally suggested the break in the contradictions. Acting on a hint given by the theory of refraction, Heisenberg was led to the suggestion that the contradictions of atomic theory would disappear if one adopted the idea of non-commutative algebra in dealing with the motions of electrons in an atom. Then the floodgates broke and the whole New Quantum Theory burst forth. It would of course be an incomplete account of it not to mention the quite different approach made independently by de Broglie and Schrödinger. If we are to trace this to its origin we must go back a century to Hamilton, for it was his work in geometrical optics which showed how a wave of short wave-length could be treated as a ray. It was de Broglie who worked out the modern analogies, but it was Schrödinger who succeeded in giving its full form, and by the invention of the *wave-function* placed in the hands of the mathematicians the most powerful of weapons for the technical discussion of atomic problems.

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At first the work was of a formal kind, obviously right, and a complete synthesis of the rival doctrines of particle and wave mechanics, but there is a very interesting point that has gradually emerged in connection with the discovery. In his first paper Heisenberg laid great stress on the idea of building theory only on directly observable quantities. It is not very clear how the distinction was drawn. The electron's orbit is certainly not observable, but is it less so than the electric force which is the amplitude in the light-wave emitted by the atom? It has seemed to me that it was not this idea of using the observable that was the merit of his work, but rather the contrary—the capacity for carrying through a formal mathematical analogy without ever asking what it all meant in terms of observable things. However that may be, it was only a year later that he remedied the defect by making a picture of his process by means of the Uncertainty Principle. I may remind you that the Uncertainty Principle asserts that it is impossible simultaneously to measure the position and velocity of any body, because the measurement of either inevitably produces a change of indeterminate amount in the other. The subject has been so often discussed that I am not going into it now, but as it concerns the centre of my argument, I want to emphasize its negative side, which, as I think, is much the most important. In this role the Uncertainty Principle is to be regarded as the argu-

ment used to defeat the old-fashioned physicist who claims that there is at any rate ideally no limit to the accuracy with which both position and velocity can be simultaneously measured. He has to admit the correctness of experiments such as the Compton effect, and we show him that by his own admission he will be defeated. On the positive side the principle is not so useful, because once we have seen the reason for the failure of classical ideas, we had better take advantage of the full technique of the quantum mechanics. Here my point is that the Uncertainty Principle showed up a fallacy in the old arguments about causality, and it was a fallacy about which we were so unconscious that we did not even know we were making it. It is now easy to see that there was nothing wrong with the old inference that if I know all about the present I can forecast the future exactly; the trouble was the impossibility of knowing the present. Once this is seen the whole argument becomes obvious, but nobody saw it until Heisenberg. We had somehow to avoid the compulsory causality of the old mechanics, and there seemed no loophole allowing us to do so until the Uncertainty Principle. Knowing what we now know we may ask why no one discovered the loophole by applying a strict analysis, for example by the use of symbolic logic. Such an analysis would presumably have revealed the fault, but the trouble is that it would also have revealed other unwarranted assumptions which we have made but which we do not in the least want to doubt, so that it would not really have helped in pinning down the exact point of error. It is invention, not criticism, that leads to the advance of knowledge.

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FOLLOWING up the later history of the subject, the success of Heisenberg in exploiting the idea of observables for atoms seemed to repeat the brilliant success of Einstein twenty years earlier in using the same idea over relativity. It seemed to imply that what was wanted in physics was to free ourselves of all abstractions and only make theories about real things. There grew up a great cult of doubting the reality of unobserved things, and then a curious thing was found; the charm did not work again, and only a few minor things have come out of it. The work of the New Quantum Theory has in fact run most surprisingly in the opposite direction. The technique is largely concerned with wave-functions, which are quantities much more abstract than anything in classical mechanics. There is certainly nothing observable, or even picturable, about waves propagating themselves in many-dimensional space with absolutely unknowable phase, and with intensity controlled by the curious extraneous rule of normalization. Largely by the use of these wave-functions the whole of atomic physics has been reduced to order, and so has molecular physics, except that it yields problems in which so many electrons are interacting that a full discussion is not feasible. So the doctrine of theorizing only about observables was not really a useful doctrine; it merely provided a germinating idea. In fact we may well ask what an observable is, and if we go at all beyond

direct sensations, which as physicists we certainly intend to do, the answer becomes perfectly indefinite. This opinion I heard admirably expressed a few years ago by the late Prof. Ehrenfest. It was in a physics meeting in Copenhagen and someone was proposing a way out of certain difficulties which involved, as he maintained, a reversion to the cult of the observable. Prof. Ehrenfest said: "To believe that one can make physical theories without metaphysics and without unobservable quantities, that is one of the diseases of childhood—*das ist eine Kinderkrankheit*."

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I HAVE dwelt at some length on the history of the quantum theory because I think it serves as an analogy to the deeper question of what is wrong with the old logical processes. Just as we used to feel the all-pervading compulsive force of causality, so we feel the all-pervading force of pure logic. Just as we felt that classical mechanics provided no room for anything beyond itself, so we feel that the old logic is the only admissible kind of reasoning. We knew that certain things led to the Old Quantum Theory and obstinately refused to fit into mechanics, and we know that the principle of probability can cover many things outside the old logic. Many men tried to force the quantum into the classical system, and many are still trying to bring probability within the fold of the old logic. I do not believe it can be done. This is not the occasion, nor have I the capacity for a deep argument on the place of probability in logic, but one of the most convincing ways of seeing it may be found in the consideration of another branch of physical theory, the kinetic theory of gases.

In the early days of kinetic theory the central problem was the law of distribution of velocities of the molecules and attempts were made to prove the law absolutely from dynamics, but the process always failed. Maxwell made the assumption that with the lapse of time a system of molecules would pass through all possible phases. There are technical difficulties in the discussion of this assumption which have never been overcome, and it is quite uncertain if it is even true. Indeed Kelvin, who disliked the whole kinetic theory, argued with some force that the only examples anyone could give contradicted the principle—for example, the motion of the planets. The greatest contribution to the subject was that of Gibbs, who recognized that there had to be a big assumption somewhere and made it quite frankly and without attempt at justification. The works of Gibbs are not easy reading; in both his great works he attends to every detail with a particularity that is really rather tedious, whereas his basic ideas are thrown at the reader almost without explanation. The idea of a canonical ensemble is a really beautiful idea once you understand it, but where does it come from? An ensemble is an idea which will be unfamiliar to many, so I had better explain it. We want to know something about the behaviour of a complicated system composed of a great many parts; say we want to know the pressure of the gas in some vessel. If we tried to attack the question by pure

mechanics, we should be faced with an enormous number of mechanical equations for the motions of the molecules, and even if these could be solved the solution would be of no use, because it would depend on the initial positions and velocities of the molecules, and these we should not know. Instead of trying this impossible and useless task, Gibbs considers a very large number of possible states of motion of the set of molecules, which have some character in common such as their total energy, but which are otherwise unrelated. Though each specimen of the motions is quite independent of all the others, he looks at them all together; this explains the word *ensemble*—I do not know why he had to take a French word—and makes the assumption that the pressure of the gas is correctly given by the average of all the specimens. The actual gas in the vessel at any instant is one of the specimens; in its motion it passes into configurations corresponding to others, but only after a fantastically long time would it go through even a perceptible fraction of the whole ensemble. Gibbs is assuming that the behaviour of the actual gas will be determined by the average of the uncountable millions of specimens in the ensemble. Almost at the start one finds oneself presented with the ensemble with hardly an attempt to explain where it comes from or why it is right, and the beginner is usually troubled by the fact that, though the subject is obviously mechanical, all the mechanics he laboriously learnt in his youth seem to have faded into comparative unimportance. There are various kinds of ensemble, the chief of which is the canonical, corresponding to all the possible motions of the gas which would have the same temperature. Later, almost as a concession to human frailty, Gibbs introduces the micro-canonical ensemble, composed of much fewer specimens because they all have exactly the same energy. This is usually welcomed by the beginner because it seems closer to his familiar mechanics, but with more experience he will realize that the gap is still so great that he is really no better off, and he may as well accept the more general idea at once.

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WITH the old mechanics all this involved ideas which for many readers were distinctly hard to accept. The principle of probability, embodied in the averaging over the ensemble, was frankly laid on top of the logical principles of Newtonian mechanics, and to anyone believing that probability would ultimately be brought down to the old logic the association was most repellent. But we can now see that Gibbs was a prophet far ahead of his time—and indeed, to be frank, far ahead of his own knowledge—for the new mechanics accommodates the ensemble very much more easily than did the old. The new mechanics has shown us that it is impossible to know how the individual molecules are moving, because when one undertakes an experiment to see, that experiment automatically alters the condition of the gas and so fails to tell what was wanted, the state of the molecules without the experiment. In the old days one used to feel that the validity of Gibbs's idea would be

spoilt by some skilful experimenter who would really observe the motions of the individual molecules and would therefore rule out the legitimacy of averaging over the *whole* ensemble, but we now know that there is no danger of this. The real gas in the vessel is not merely one specimen of the ensemble, unrecognizable only because of our clumsiness; it is itself the whole of the ensemble. We used to think of the gas as *either* in the state A, *or* in the state B, *or* in C, but according to the new physics we have to think of it as in *all* the states A *and* B *and* C. The distinction is typical of the change we must make in our habits of thought, and most of us resist this change strongly, for we find we can hardly help asking: "But which state was it really in?" As I have said, we used to be ashamed of ignorance, but we must now realize that *this* ignorance is one of the things that makes the world possible. The principle of probability, which used to be loosely superposed on the old logical principle, is now with the new mechanics fully united with it in a higher synthesis.

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BEFORE leaving Gibbs I would like to refer to one thing in his book, where I think he has not even yet come into his own. He considers various types of ensemble of increasing generality. In the micro-canonical the members all have the same energy. Now we never know the exact energy of the gas in a vessel, so that a better idea is the wider one of a gas at a given temperature which therefore has a certain range of admissible energies. This is represented by Gibbs's canonical ensemble, and it is the main one that he uses. In both these the number of atoms in the ensemble is constant. But in the last chapter of his book Gibbs introduces a still wider ensemble. He calls the ones with a constant number of atoms *petits ensembles*, which I shall translate as petty ensembles, and regards them as parts of a grand ensemble in which the total number of atoms is not fixed. He uses the idea to some extent in connection with semipermeable membranes, but on the whole does not get far with it. As in much of Gibbs's work, it is the idea itself, rather than what he does with it, that is important. This idea of the grand ensemble is not yet incorporated in the new physics. In the quantum theory we take a number of electrons and nuclei and, allowing for their interactions, we construct something that is practically the canonical ensemble. But we take fixed numbers of them—this is partly reflected in the technical process of using normalized wave-functions. Now in an experiment dealing with a large number of particles we are never really sure exactly how many there are, and to assume this number is much like assuming a constant energy for them. If the canonical ensemble is a better idea than the micro-canonical, then the grand ensemble is superior to the petty ensemble. In the new mechanics nobody has yet succeeded in making anything of it, or has made any proposal how to do so, but I will venture the forecast that when some of our present difficulties in the quantum theory are cleared up, it will be found that we shall be using the grand ensemble with its indefinite number of atoms.

Reverting to my main theme, what is the moral of all this? It is that the new physics has definitely shown that nature has no sharp edges, and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline. In the old days it looked as if the world had hard outlines, and the old logic was the appropriate machinery for its discussion. Things went wrong when it was found necessary to call in the help of the principle of probability; this appeared first as an alien, but there was hope in the old days that the alien might be naturalized. It has resisted the process and we now recognize that it cannot be assimilated, because it provides the necessary step to a wider reason, that of the new fuzzy world of the quantum theory, a world which is not contained in the old. How far it will be possible to make a full synthesis of the new and the old I do not know, but I like to think there is something in my analogy from the history of the quantum theory, and to suppose that we are still in the condition corresponding to the Old Quantum Theory, and that some day a real synthesis will be made like that of the New Quantum Theory, so that there will be only one thing in the world that has not indefinite outlines, and that will be a new reformed principle of reasoning.

I may fitly conclude this part of my subject by returning to the point from which I started. As an example of what the ordinary man regards as correct reasoning I quoted some words of Sherlock Holmes. I must not confess that I was not quite sincere in my quotation; the impression I gave was the impression the reader carries away, but on examining the text I was interested to find that the great detective had himself arrived at the ideas I have been putting forward. In the sentence before he said: "No, no; I never guess. It is a shocking habit destructive of the logical faculty", he had said: "I could only say what was the balance of probability—I did not expect to be at all accurate." The master-mind uses the word logic in its modern sense.

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THERE may be a feeling among some that the very general suggestions I have been making are open to every sort of criticism. Perhaps they are right; as I have said, it is part of my doctrine that the details of a physicist's philosophy do not matter much. But whether it is wrong or right, my next point is one on which I do very much hope that there may be a consensus of agreement. This is that the subject of probability ought to play an enormously greater part in our mathematical-physical education. I do not merely mean that everyone should attend a course on the subject at the university, but that it should be made to permeate the whole of the mathematical and scientific teaching not only at the university but also at school. To the best of my recollection in my own education I first met the subject of probability at about the age of 13 in connection with problems of drawing black and white balls out of bags, and my next encounter was not till the age of 23, when I read a book—I think it was on the advice of Rutherford—on the kinetic theory of

gases. Things are better now, but mathematicians are still so interested in the study of rigorous proof, that all the emphasis goes against the study of probability.

Its elements should be part of a general eduction also, as may be illustrated by an example. Every month the Ministry of Transport publishes a report giving the number of fatal road accidents. Whenever the number goes up there is an outcry against the motorists, and whenever down, of congratulation for the increased efficiency of the police. No journalist ever seems to consider what should be the natural fluctuations of this number. A statistician answers at once that the natural fluctuation will be the square root of the total number, and apart from obvious seasonal effects that is in fact about what the accidents show; the number is roughly 500 ± 25. The proof of this does not call for any difficult mathematics, neither the error function nor even Stirling's formula, but can be done completely by the simple use of the binomial theorem. There is no mathematical difficulty that should trouble a clever boy of 15; it is only the train of thought that is unfamiliar, and it is just this unfamiliarity that is the fault of our education. The ideas and processes connected with the inaccuracy of all physical quantities are much easier to understand than many ideas that a boy has to acquire in the course of his studies; it is only that at present they are not taught, and so when met they are found difficult.

This is not the place to describe a revised scheme of education. I would only say that it is not special new courses that are needed, but rather a change in the spirit of our old courses. When a boy learns about the weighing machine, emphasize its sensitivity, and consider the length of time that must be taken for the weighing. When he has a problem on projectiles, make him consider the zone of danger and not merely the point of fall. At a rather higher level, but still I should hope at school, introduce the idea of a distribution law; for example, in doing central orbits work out Rutherford's law of scattering. Calculate the fluctuations of density of a gas, or the groupings in time of the scintillations of α-particles. All these things ought to be examples of a familiar train of thought, and not merely a highly specialized side branch of mathematics first met at the university. It is the incorporation of probability in the other subjects on which I want to insist, but there will of course remain some higher aspects—things like least squares or significance tests—which are still to be treated in separate university courses. Even these I should hope would come to be recognized as subjects of central interest and not, as they are at present, relegated to a remote corner of specialized study.

If these reforms are carried out I shall hope that generations will grow up which have a facility that few of us at present possess in thinking about the world in the way which the quantum theory has shown to be the true one. The inaccuracies and uncertainties of the world will be recognized as one of its essential features. Inaccuracy in the world will not be associated with inaccuracy of thought, and the result will be not only a more sensible view about the things of ordinary life, but ultimately, as I hope, a fuller and better understanding of the basis of natural philosophy.

The Changing Outlook

By Professor R. V. SOUTHWELL

President of the Engineering Section of the British Association

BRIEFLY, my thesis is that we should avoid undue humility! The times are out of joint, and having attained to command of Nature greater than the world has seen before, because man has not learned to use his mastery wisely, illogically now (as it seems to me) he inclines to question the value of that mastery, and the labours that have given it. In particular I want to record my protest against what seems to be an implication in much that is written nowadays, that because the range of engineering includes guns, battleships, aeroplanes, tanks, therefore engineers are to be regarded as a class more than others responsible for the horrors of modern war.

Here are words spoken by Sir Alfred Ewing, in a presidential address to the Association (1932) which I keep to read ever and again, for its showing of what at the best an engineer's outlook may be:

"An old exponent of applied mechanics may be forgiven if he expresses something of the disillusion with which, now standing aside, he watches the sweeping pageant of discovery and invention in which he used to take unbounded delight. It is impossible not to ask, Whither does this tremendous procession tend? What, after all, is its goal?...

"The cornucopia of the engineer has been shaken over all the earth, scattering everywhere an endowment of previously unpossessed and unimagined capacities and powers. Beyond question many of these gifts are benefits to man, making life fuller, wider, healthier, richer in comforts and interests and in such happiness as material things can promote. But we are acutely aware that the engineer's gifts have been and may be grievously abused. In some there is potential tragedy as well as present burden. Man was ethically unprepared for so great a bounty....The command of Nature has been put into his hands before he knows how to command himself."

Here too are words spoken somewhat earlier, in his wonderful James Forrest Lecture, 1928, on "A Century of Inventions". In them still more clearly, as I read them, he seems to feel as engineer a sense of special responsibility:

"I used, as a young teacher, to think that the splendid march of discovery and invention, with its penetration of the secrets of Nature, its consciousness of power, its absorbing mental interest, its unlimited possibilities of benefit, was in fact accomplishing some betterment of the character of man....But the war came, and I realized the moral failure of applied mechanics....We had put into the hand of civilization a weapon far deadlier than the weapons of barbarism, and there

was nothing to stay her hand. Civilization, in fact, turned the weapon upon herself. The arts of the engineer had indeed been effectively learnt, but they had not changed man's soul....

"Surely it is for the engineer as much as any man to pray for a spiritual awakening, to strive after such a growth of sanity as will prevent the gross misuse of his good gifts. For it is the engineer who, in the course of his labours to promote the comfort and convenience of man, has put into man's unchecked and careless hand a monstrous potentiality of ruin."

To which I personally would answer: "Yes, for the engineer as much as any man, *but no more*." And when, in more recent pronouncements, I find the charge so glibly formulated—"It is engineers who have given men these potent weapons of destruction: on them *more than others*, then, rests the responsibility for their use"—then, admitting the premise, I protest against the deduction. I would say rather: "On them as much as on others (but no more) rests the responsibility for their use." Do not think that I imagine the load thus shared will be light for all. I have no illusion about the weight of responsibility—it is appalling; but I hold that we must share it equally, as citizens, not look for scapegoats when we have been free either to choose our path or leaders to direct us.

* * * *

I can conceive no subject in which, more than this, clear thinking is wanted to-day: the desire to hand on responsibility is so deep-seated, and the will to believe that we could have had the benefits of science without its risks and its temptations. But knowledge is of good and evil: it is of its essence that we cannot know how to cure poison without knowing poison and its action, how to control and use explosives without acquiring power for harm as well as good. We may elect either to shun it or pursue, but we cannot have it both ways. Either we must choose, deliberately, impotence as preferable to the power of doing evil, or we must accept knowledge for the double-edged tool it is, vowing to use it wisely. We may not say to the scientist, "Keep searching, but let your discoveries be such as must benefit and cannot hurt us"; or pretend that the use we make of science is something outside our responsibilities as citizens, a thing imposed upon us by science itself. *Knowledge is not moral*: good and evil are its opposite sides, inseparable in its very nature. I have no quarrel (though no sympathy) with the plea we sometimes hear, for a cessation of scientific activity: it is arguable that on balance knowledge is undesirable. But when men talk of "beneficent" and "destructive" science as though we were free to pick and choose, then I say that they have not even begun to understand what science is.

Holding these views, I find it matter for regret that so often our concern with the impact of science on the life of the community, which is good and healthy, is expressed in a manner that is neither. Too often we seem to be weakly apologizing for results that have followed our activities, as it were because we did not take

sufficient care. Need the geneticist apologize for having increased the earth's fertility, because we have found no better use for plenty than to destroy food while thousands are in want? Ought doctors to regret that by coming to a fuller understanding of disease they have lengthened the span of life in a world where birth rates are falling? Here and in countless other instances, science impinging on the life of the community has set problems that will tax to the utmost its courage and intelligence; the hardest and clearest thinking will be wanted, and it is right that engineers and scientists should seek to contribute their share. But I think that we only confuse the issue when we intervene *as specialists* in discussions which concern us really not as specialists but as members of a community.

* * * *

Whether in these days, when all but a small minority seem convinced of the necessity of rearmament, the engineer is still regarded as scapegoat or has (for a time) been transferred to the role of saviour, I have no means of judging. But if any still reproach him for making what all men now seek to buy, I would answer that horror is not peculiar to modern war; all war is horrible, both in nature and by purpose, and wars are made not by engineers but by communities. No war is righteous, though it seem so at the time; or inevitable, except as a penalty of national sins: pride, greed and indolence; and those more contemptible because weaker sins, vacillation of purpose, persistence in shams, clinging to safety even at the loss of honour.

More and more frequently, in lectures and in editorials, the decline of international standards is noticed with consternation and lament. Naturally, perhaps, in this country we are apt to see it mainly as an increasing tendency towards "repudiation of law and order in favour of brute force",* revealed most clearly in states that have abjured the democratic ideal. But I think that the malady is at once deeper and more general. Dare we claim that our own policy has shown no falling away from earlier belief in straight-dealing, generosity, and the sanctity of contracts?

Increasingly, as it seems to me, nations incline to put trust in the adroitness rather than the sincerity of their statesmen. Ethics are out of fashion, and while as individuals we may still admit the moral imperative, the notion that motives recognizable as moral can have place in international affairs seems now to be rejected as impracticable idealism. Force and deceit, it appears, although unpleasant are held to have "survival value": the gangster compels our unwilling admiration, at least in the field of world affairs. But what if there should be something in the notion, that because success in the life-struggle can come not only by individual strength but also by ability to associate and combine, morality has survival value as being (thus regarded) one of the factors which make association possible? A bank may come to ruin not only through fraudulent or incompetent direction, but because its depositors, panic-stricken, seek each his own legitimate interests at the

* Vide *Nature*, 28 May, 1938.

expense of the common weal: may not a less narrow concept of moral obligation be necessary to the continuance of our civilization, even as wider than national horizons are necessary in the spheres of economics and finance? Perhaps this "idealism" is not so impracticable after all?

Collective security attained by higher standards of fair dealing—it is an epitome of man's progress from the cave to association in the village, in towns, and in nations, and I see no ground for believing that the notion can never transcend national barriers. Men write as though it were new—a product of post-armistice utopianism. That it is not new let these sentences, none written less than 100 years ago, bear witness (Guedalla, 1931):

"Soyez sûr qu'en politique il n'y a rien de stable que ce qui convient aux intérêts de tout le monde; et qu'il faut regarder un peu plus loin que soi-même."

"...although the aggrandizement and security of the power of one's own country is the duty of every man, all nations may depend upon it that the best security for power, and for every advantage now possessed, or to be acquired, is to be found in the reduction of the power and influence of the grand disturber."

"If we lose our character for truth and good faith, we shall have but little to stand upon in this country."

"I would sacrifice Gwalior, or every frontier of India, ten times over, in order to preserve our credit for scrupulous good faith, and the advantages and honor we gained by the late war and the peace.... What brought me through many difficulties in the war, and the negociations for peace? The British good faith, and nothing else."

If this be utopianism, then some of our historical judgments will need revision: for all were said or written by Arthur, Duke of Wellington—a man not lightly to be charged with saying what he did not mean.

You will say, now I am drifting perilously near to politics! It is precisely the point I want to make: I say that inevitably, when instead of science we discuss its impact on the life of the community, we *must* verge on politics, because what concerns the community *is* politics, both etymologically and in fact. The old convention, that science should have no politics, seems to me sane and wise: how to preserve it if as scientists we are to concern ourselves with the life of the community, that is a question I must leave to others more subtly-minded. For myself I see no reason why as scientists we should meet to discuss anything but science. Contrary to common belief, it is not our habit to pursue science throughout the whole of every day; and on all counts I hold it were better that we came to political discussions in hours of leisure, unlabelled, than give support to a notion that political problems will yield to something known as "the scientific attack". Talk to me of the scientific approach in physics, and I shall have an idea of what you mean, though you will easily bewilder me with detail: talk to me of "scientific approach" to problems of real life, I shall suspect you of indulgence in mere jargon.

This is not to assert that science unfits a man for political discussion: if only because by training men of science are prepared to believe that problems of urgency may yet be hard, I hold on the contrary that some scientific leaven is beneficial in almost any body of administrative humanists. It is a protest against our facile modern use of the word "scientific" (which if it means anything connotes a special kind of approach to special problems) where "trained common sense" is the faculty which is really needed. In science we seek to explain phenomena which we believe to be outside man's control: it is the faith in which our work is done—for if the facts were not inexorable, and could be altered at man's pleasure, how could we hope to find enduring "laws"? But politics is concerned with action in fields where we believe that we can influence results: I see no reason to believe that the same technique will serve.

Science Against Crime

By Nigel Morland

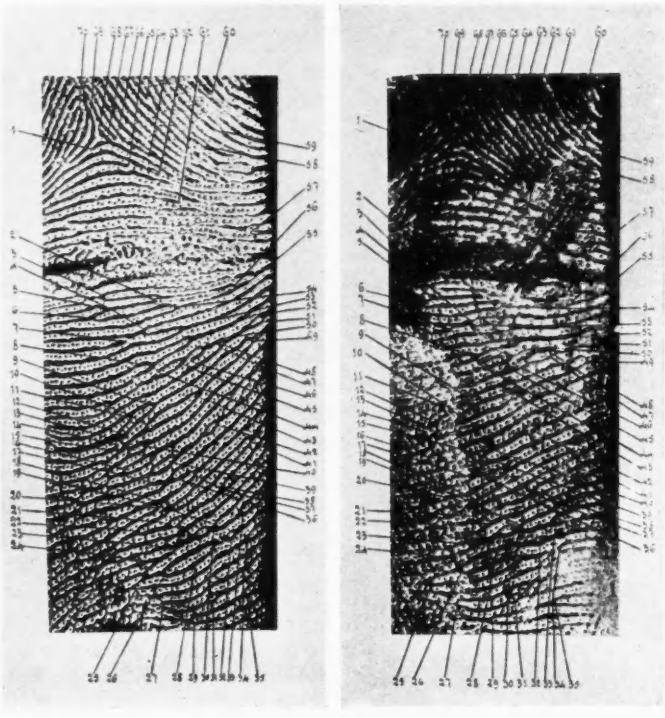
Readers of crime reports in the newspapers cannot fail to have noticed the increasing frequency with which experts from the Hendon and provincial police laboratories give scientific evidence, even in comparatively minor cases. The result is a growing recognition of the important part science is playing in circumventing the criminal. That this part is no longer confined to the purely medical side is revealed by the variety of topics on which expert testimony is to-day given in the courts.

Even these indications, however, fail to do justice to the immense importance of scientific criminology in the work of the police. There is practically no branch of knowledge that does not now make its contribution to detection, and there is, too, a great deal of original research going forward that is not only placing new weapons against crime in the hands of society, but also adding to the general knowledge.

Fingerprints are the most widely used and certainly the best-known method of scientific identification. This system, as is well known, depends upon the unique individuality of the ridge patterns on fingers and thumbs, and their persistence throughout life. Since the system was adopted in this country, which set the lead in this respect, it has been refined in various ways, notably in the case of the single-fingerprint system developed by Superintendent Battley of Scotland Yard. This renders the identification of a person from a single print, or even part of one, comparatively easy.

Apart from its practical success, fingerprinting is a very striking commentary on that extraordinary and mysterious thing we call individuality. It points unmistakably to the fact that no two people are exactly alike. Since the basis of all police work is positive identification, it naturally results in attention being directed to the examination of other characteristics of the human body in which this same persistence of individuality shows itself.

Perhaps the most remarkable of these



Identification by Poroscopy

Method by which pore patterns are compared and identified (pattern on right comes from the scene of the crime; pattern on left was taken after arrest).

developments is poroscopy, for which police science is indebted to Dr Edmond Locard, director of the Laboratoire de Police Technique at Lyon, and perhaps the most original mind in modern criminology.

If a fingerprint impression is examined, it is found that the ridge pattern is broken up by minute, roughly circular markings. These indicate the position of the sweat pores in the skin. Their number is so vast that, on the theory of possibility alone, it seemed unlikely that any two people should have exactly the same distribution. Experimental work by Dr Locard showed this to be true; and poroscopy has been adopted, at least in France, as a valuable aid to fingerprint identification.

The merit of this system is that it can be applied with success when a fingerprint record is incomplete and can give at best no more than a partial identification. Thus, in one case Dr Locard handled, a series of fingerprints on broken glass gave between them an insufficient total for positive identification. Poroscopical examination, however, yielded no fewer than four hundred points of identification from a fragmentary little-finger print alone; this, coupled with many other points of identification from other prints, brought the case to a successful conclusion. In another case, identity was established, through poroscopy, from a print made on a pellet of candle wax no larger than a grape pip.

Another offshoot of the fingerprint system is kneecap printing, which is being investigated closely in the United States. Here again, results seem to indicate that the patterns on the kneecaps are as individual and persistent as those on the fingertips and the distribution of the pores.

One of the questions frequently raised in connexion with fingerprints is whether they can be forged, and this aspect recently received widespread publicity in the courts. The argument runs that, since the fingerprint is no more than an impression of a

A highly magnified photograph of a finger print

series of lines, copying by photographic means should be simple and accurate.

Generally, this argument is based on fact. It is perfectly feasible to make an exact copy of a fingerprint by photographic processes broadly similar to those adopted in printing for the production of line blocks. Already, in a detective novel published

more than thirty years ago, attention was drawn to possibilities in this direction. Yet even if this is granted, it does not follow that the forgery will be successful in deceiving the expert examiner.

Whether such a reproduction could mislead an experienced investigator has always been doubtful, and recent progress in what is called the "development" of prints—i.e. their working up with various media for examination—makes successful forgery still more remote. Examination by ultra-violet and infra-red light is important in this respect. The forged print usually betrays itself by a certain indefiniteness of impression.

Analogous to mechanical forgeries are the attempts made in the United States by which a new skin pattern is surgically grafted on to a person's thumb or finger. This method is said to be successful for a year, but it is not wholly so in its aim. If a rolled print be taken—that is to say, if the finger is rolled from one edge to the other so as to give an impression of its whole area—the joints of the grafting are clearly visible. Endeavours to destroy fingerprint patterns, as in the famous case of Dillinger, are equally unreliable.

Undoubtedly the most remarkable example of the scientific reconstruction of identity was the Ruxton case; the enthusiast can study this in its full report and always find some new and striking aspect. Here, the investigators were faced not with the identification of any living person but with that of a collection of human remains discovered wrapped in four bundles in a stream in Scotland. The subsequent work, which cannot be adequately dealt with in a brief article, revealed amazing subtlety and ingenuity on the part of the experts concerned. Not only were the remains separated into two different bodies, but a fair measure of agreement was obtained between estimated bodily dimensions of the skeletons and those of the two missing women. Photography was utilized to superimpose the features of the missing women on the skulls discovered and again indicated great similarity; and palmar prints were developed to provide further evidence. The whole was a triumph of which British scientific criminology may well be proud.

The various methods of identification of persons are so many and fascinating that it is difficult to leave them; but not all problems of detection are concerned with this subject, vitally important though it is. A large bulk of police work consists of the examination of inanimate objects with a view to establishing their connexion with a specific person or place.

No science has made greater progress in recent years than ballistics, which originated in the United States. In that country, some means of connecting bullets with a given gun assumed the highest importance owing to the large number of shooting crimes, and the science became the subject of intense study.

As fingerprints and allied methods show the individuality of persons in minor details, so forensic ballistics has shown a similar state of affairs in mechanical products. It has been found that in the machining of gun barrels and breech locks the tools leave quite individual marks, and that, further, no two weapons wear in exactly the same way.

When a cartridge is fired, it is forced back against the breech block and the soft metal of the shell takes up the tool and wear markings. This impression is, significantly enough, called the "thumbprint of the gun". If it is desired to discover whether a certain cartridge was fired from a given gun, test shots are fired from the latter, and the markings on the test cartridge and the suspect are microscopically compared. Identity, when established, is virtually incontrovertible.

Most ballistics cases, however, are concerned with the examination of bullets found on the scene of the crime. The material of which bullets are made is soft as compared with the rifling of the gun barrel, with the result that it takes up the tool and wear marks of the barrel, just as the cartridge shell takes up the marks of

the breech block. Thus, there is a thumbprint of a bullet, as there is of a human being or a gun.

For this work, the comparison microscope is employed. This is an instrument that enables two objects—in this case the two bullets—to be seen under magnification in the same field and their characteristics to be accurately compared.

More recently, a development of technique is tending to reduce the importance of the comparison microscope. Rolled impressions of bullets under test are made in a plastic material and comparison is then made by spectroscopical means. Spectroscopical examination is also proving of value in the identification of powder grains, another important aspect of forensic ballistics.

In modern physics, examination of objects under ultra-violet light has proved of immense value. Scientific criminology is also turning this method to valuable practical account. It is specially useful in such matters as disputed documents and the discovery of bloodstains.

Examined under ultra-violet light, alterations in documents that are undetectable to the unaided eye are clearly shown up, particularly in connexion with the microscope. Bloodstained material that has been washed to such good effect that the stain seems to have completely disappeared reveals its secret when the same plan of examination is adopted. The field here is so immense that only two of its many applications can be mentioned, but those familiar with what examination by the so-called "invisible light" has achieved in physics cannot fail to appreciate its significance in criminal investigation.

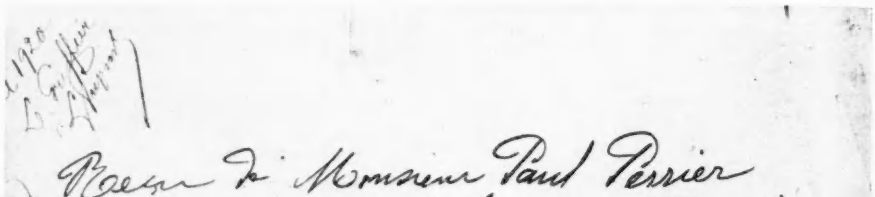
Microscopy and chemistry combined form one of the chief weapons science uses in its war against crime. By their aid, the most minute and seemingly useless clues can become of primary importance.

Here are just a few of the achievements in this particular sphere: hairs can be identified with reasonable certainty, particularly as to the part of the body from which they originate. Blood tests have become a commonplace of criminal investigation, though their evidence is, as a rule, negative—that is, while they can establish in certain cases that a stain was not made by a given person's blood, they cannot establish that an individual specimen belongs to a specific person. Documentary forgeries are practically helpless in the face of microscopical examination, and chemistry can reveal much as regards ink and paper.

Dust identification is another line of inquiry that has proved invaluable in many cases, since the record of a person's wanderings can often be deduced from the dust his clothes carry. More suggestive of the detective novel rather than actual fact, criminals have been brought to justice through the analysis of tobacco ash and even lipstick traces.

Photomicrograph of ordinary dust from a road

Little has been said here of the medical aspect of criminal investigation for the twofold reason that the province is so vast that it can hardly be discussed generally and that it is comparatively well known. Medical jurisprudence, or forensic medicine, does, however, form the very basis of criminal investigation and had become


Reçu de Monsieur Paul Perrier fermier ma quatre la somme de six cent soixante frs. pour le restant de la ferme échue pour le 1er Novembre
Montoison le 1er novembre 1919.
Casimir Perrier

A Forged Receipt
The last figure in 1917 *has been altered to* 9 *(as shown in the photomicrograph on the opposite page)*

established as such long before other branches of science were pressed into service.

The detective is always being faced with fresh problems. Just as science helps him in his work, so it also raises new difficulties by opening up fresh resources to the criminal. The experience of the police in the U.S.A. is illuminating in this respect. In that country, organized crime has not been slow to take advantage of the march of science, with the result that the work of justice has sometimes been considerably hampered. One example is the use of plastic surgery for the alteration of a man's features. Means of circumventing this device have been successfully developed, for one physical attribute is always ignored by plastic surgeons. This is only one of the factors which gives the lie to attempts at facial disguise.

One problem of particular interest to this country is the fact that modern scientific criminology falls into so many specialized sections as to make the full utilization of its resources a matter of some difficulty. In other countries, the difficulty is solved to some extent by the existence of central institutes of medical jurisprudence and the like, which act as both training centres and research laboratories for the whole police force. The need for such an institute in Great Britain has often been stressed and, indeed, recognized by a committee set up to consider the subject. But it was recently announced that the time was not opportune for its creation. This particular problem therefore remains and cannot but become more grave as scientific methods increase.

This point leads to another that is worth discussing in conclusion. Is criminology a science? Is it not rather the practical application of the results of other sciences in one specific field? One answer to this is that it has as much claim to be considered a science as medicine, which to-day is based on the findings of research in specialized fields. Another answer is that it is to a large extent built up on individual work

that does not fall under any of the recognized heads. Fingerprinting, forensic ballistics, and the various methods of identification, are the outcome of special investigations carried out by criminologists. If certain aspects of their technique are common to other sciences, that does not in any way invalidate the specific nature of the work.

Moreover, it is clear that this special research, while drawing from other fields, repays its borrowings by significant contributions, the value of which is often overlooked in the more general survey. There is surely something of the highest importance to both biology and psychology in the growing evidence criminology brings forward of the persistence of individuality. Every modern method of identification is based on this fact; and the more the knowledge gained, the greater is the amount of proof adduced in support of it.

The aim of criminology is to catch the criminal. With that object, it turns all things to its purpose. But the fact remains that, if by some miracle crime could be swept away, the vast accumulation of data in the criminological archives of the world would be found, if properly sifted, to suggest some very curious and striking facts of value to the cause of "pure" science.

Photomicrograph of the forged figure 9 in the date of the receipt shown on the opposite page

Controlling Infantile Paralysis

By Andrew R. Boone

Scientists engaged in the fight against Infantile Paralysis are hopeful that at last they have the weapon with which to control this withering scourge of childhood, youth and early maturity. The search for the right weapon has been long, discouraging and greatly handicapped by lack of complete knowledge of both the way in which the disease travels from person to person and the way in which it operates to destroy the nerve-cell nuclei in the spinal cord.

Until as late as April 1935 pathologists and bacteriologists hoped to conquer the ultra-microscopic virus of poliomyelitis by vaccines or serums. In the laboratory they were inducing paralysis in Rhesus monkeys by virus inoculations, then drawing from the blood of the convalescent monkey a serum in which their hopes for successful immunization were placed. But discouragement followed. The serum so produced was injected into other monkeys, which were then inoculated with the virus. Seventy-five per cent of them came down with the disease, notwithstanding. Absence of infection in only 25 per cent of the monkeys was not satisfactory; something must be wrong with the theory of the disease or else serum would put up a better fight against its virus enemy.

Dr E. W. Schultz, professor of bacteriology and experimental pathology at Stanford University, brought this disconcerting news to pathologists, bacteriologists and immunologists at a meeting in New York, 18 April 1935. His report, written in collaboration with his associate, Dr L. P. Gebhardt, concluded that the production, through serum, of fighting antibodies in the blood would never be likely to avail much against the polio virus.

This was certainly enough to becloud every polio worker's hopes for victory. But discouragement was not defeat. The very proof that antibodies in the blood, propagated in response to the introduction of vaccine, could not get at the destroying virus led to the logical conclusion that the virus, in its travels through the body, must somehow escape contact with the blood. To do this, the researchers reasoned, it must enter the nervous system direct, without passing through tissue where it would be subject to attack by the humoral antibodies. Now the only area of the body where nerves lie exposed to the surface is the olfactory area, far up in the nasal passage, beneath the bony cribriform plate, which is found just below the frontal lobe of the brain. Here, the researchers said, must be the portal of entry. The virus (whether transmitted freely through the air or only by actual bodily contact in areas of contagion is not known) must find its way to the little, hair-like olfactory processes that lie in the nasal mucosa. Apparently it must enter there and travel on from one level of the nervous system to another, probably inside the neurons themselves and hence out of reach of the circulating antibodies in the blood. Or, as Dr Schultz expressed it, "the virus is like a mouse scurrying along inside a drainpipe through which the cat cannot follow".

Gloom in April 1935, however, was dispelled by a new ray of hope in May. Two research men of the United States Public Health Service, Charles Armstrong and W. T. Harrison, taking a new tack, reported to the scientific world that instillations of alum into the nostrils of inoculated monkeys would prevent infection for a few

days. Others were quick to follow the new course of experimentation. Drs A. B. Sabin, P. K. Olitsky and H. R. Cox of the Rockefeller Institute confirmed what Armstrong and Harrison had observed and reported that a 4 per cent solution of tannic acid also exercised a protective action. This was a new approach to the control of polio, a mechanical approach direct to the olfactory portal of entry. Yet what practical promise it held none could say.

By the summer of 1936, it was known, through researches upon 240 monkeys in the Stanford laboratories, that zinc sulphate gave a more lasting and a higher degree of protection than alum. No gold seeker, stumbling on a new strike, could have been more elated than Dr Schultz and Dr Gebhardt were when they squirted a one per cent solution of zinc sulphate on to the nasal mucosa of 53 monkeys and watched 95 per cent of them successfully resist infection afterward. Moreover, they were encouraged to find that resistance to infection remained in the monkeys for from one to three months—a factor of great importance if the process of chemical prevention, or chemoprophylaxis, were to be transferred to man.

This, then, is the new stage of the battle. The polio laboratories, supported largely by funds contributed by the public to the President's birthday celebrations, have placed a weapon of promise in the hands of the medical profession. It remains to be determined whether this method is as effective in man as it is in monkeys. Last summer the province of Ontario, Canada, was swept by polio. The zinc sulphate method of preventing infection in monkeys had just been announced by Drs Schultz and Gebhardt. Public health officers of the province sprang into action, organized the services of nose and throat specialists and began the tremendously difficult, wholly experimental task of administering the new preventive to 5000 children.

Nasal spraying was the technique first employed in Ontario. The results were disappointing, and it is now conceded that the experiment failed to prove or disprove the effectiveness of zinc sulphate as a preventive. This was because only 25 per cent of the children treated temporarily lost their sense of smell. Loss of the sense of smell is an indication that the olfactory area has been thoroughly covered by the zinc sulphate solution, and naturally unless the area is thoroughly covered, to the extent that the smell-sense is lost, scientists cannot determine with any degree of conviction the chemical's preventive value.

Discouraging though the failure of the Toronto, Ontario, field test was to medical men, Dr Schultz revived hopes for eventual success in chemical prevention by pointing out to the Society of American Bacteriologists inWashington, D.C., last December the discovery of a new method of introducing the solution. This is the work of a group at the Stanford Medical School, and the method is simple, quick, and, from experimental results, effective. But Dr Schultz points out that while temporary loss of the sense of smell should probably be regarded as evidence of a thorough application of zinc sulphate to the olfactory processes, studies still need to be carried out to determine whether there are any risks of permanent loss of sense of smell. While thus far there are no indications that loss of it is permanent, a very few of those treated with zinc sulphate have been slow in regaining this function.

"Because of the short duration of the protection", Dr Schultz says, in summing up the present value of the treatment, "and because of the unlikelihood that it will ever be applied to a very large part of the population, it is not a prophylactic measure by which we can hope to eliminate the disease. Yet while the measure is basically not ideal, it does possess certain virtues not so likely to be found in methods which might leave a more lasting immunity. The agent is simple to prepare and inexpensive. Moreover, it is free of the risk of inducing infection. Whether it has a future or not, there is a great deal of work just ahead which must be done."

A Dowser's Questions

Sir, My attention has been drawn to an article entitled "Science and Dowsers", by Mr Oliver Gatty, in your issue for September 1938. Mr Gatty says in this: "Capt. W. H. Trinder of Brockenhurst was able to get a response to stationary water in a tumbler when he knew it was there. When he no longer knew the answer, he did no better than chance expectation." Later in his article Mr Gatty states: "It may therefore be concluded that dowsers cannot divine under laboratory conditions and against known odds."

May I state that Mr Gatty is not correct in his statement regarding my test, as the facts were as follows: A number of tumblers were placed on the floor of a room, when I was not present, and covered. One of these contained water and I was asked to say which. The actual result was that on my first attempt I was correct but on the glasses being moved I failed to get closer than two glasses away from that containing the water. The "test" took place (*a*) After a lunch party at a country house. (*b*) In a medium sized room with eight or nine people all standing round me and, in accordance with latest theories, all giving off radiations.

If these are the conditions under which Mr Gatty generally works, I am sorry for the results of his "laboratory" researches and if his other "tests" were carried out with a like disregard of laboratory conditions it is not to be wondered at that he managed to "fail" the dowser being tested. Fortunately the whole matter is now being tested by two scientists testing dowsers under real laboratory conditions, using scientific instruments instead of prejudice, and the results may cause something of a stir in the scientific world.

I would like Mr Gatty to answer the following questions:

(1) Have geologists NEVER failed to find water where they stated that water should be found?

(2) Why, unless some form of dowsing was employed, were some of the very old deep wells sunk, in many instances, in such unlikely places? A very good example of this is the well (200 ft. deep) at Carisbrooke Castle, Isle of Wight, though this is only one of many. At the time this well was sunk there were no geologists, as we understand the term, nor was there then any recorded information about the strata. Obviously, even though labour was extremely cheap at that time, no one would start to sink 200 ft. through solid chalk, at the top of a hill, unless they had some shrewd idea that there was water there. The most practical solution seems to be that they employed a dowser, or will Mr Gatty try and put this theory aside with, what appears to be his favourite conclusion, that "it is due to chance expectation"?

W. H. TRINDER

Andoversford, Glos.

Mr Oliver Gatty's Reply

The conditions of the experiment with Captain Trinder are correctly described in his letter. He found the water in the only tumbler out of twelve that contained any on his first attempt. There followed ten consecutive failures. At the time Captain Trinder attributed his failures to using pendulums of the wrong colour for that time of day, but changing the colour did not improve his performance. If he had asked me to place the tumblers farther apart I would have done so—I did not suggest that myself at the time as I had had little experience of dowsing when I tested him. Some of the dowsers I have tested, such as Major Pogson, have asked to have the jars kept farther apart, and I have then done this and even had the jars out of doors in a place previously tested by the dowser himself for a uniform response or absence of response to water. Captain Trinder has since told me that he could now distinguish between pennies and half-crowns in envelopes. I have invited him to undergo a test, but he has not done so.

If Captain Trinder cares to submit to another series of tests I am prepared to wager either £2000 to £20 or £1000 to £10 that he will not succeed at a 10,000 to 1 chance, the tests to be carried out before six independent judges. (This in addition to my standing offer of £220 to nothing for a similar chance.) The conditions of the wagers are that none of the people present knows in which jar there is water; that the water cannot be detected by the five senses of an observer; that the position of the jar containing water is mathematically randomized; and that the odds are worked out according to the classical mathematical theory of probability.

Naturally I admit that geologists fail to find water, and it is possible that a dowser was employed at Carisbrooke Castle. Whether it was luck that such a dowser was correct I am unable to decide. If Captain Trinder looks at my article he will see that I have arrived at no definite conclusion as yet about dowsing in the field.

Notes of the Month

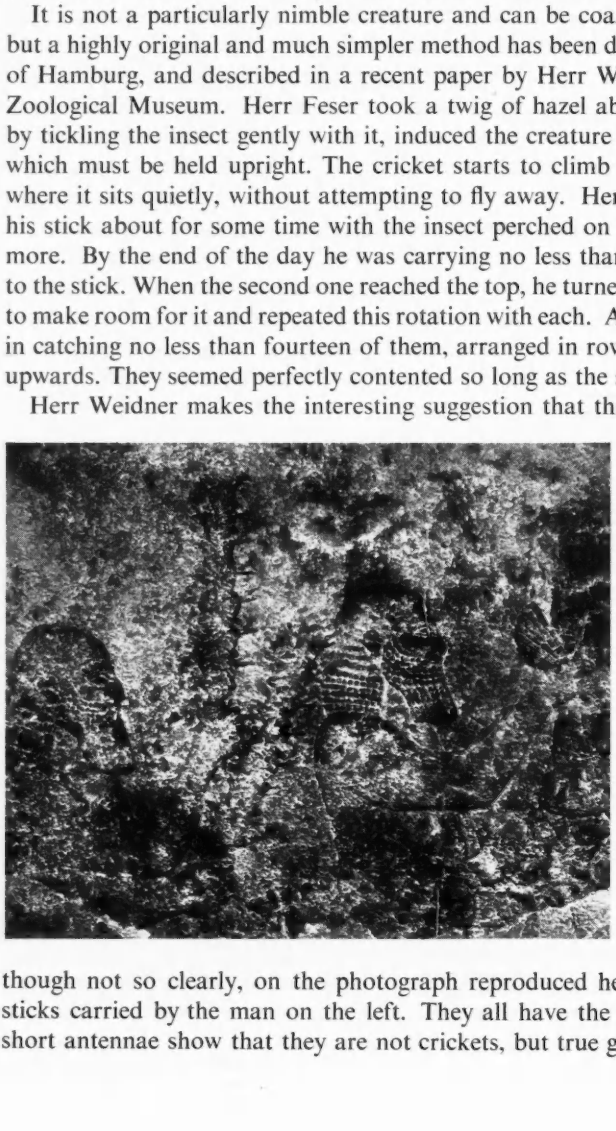
Grasshoppers on Sticks

MOST people are familiar with the Great Green Grasshopper, or, as I prefer to call it, the Great Green Bush Cricket, for it is more nearly related to the Crickets than to the Grasshoppers. (The Crickets have long, thread-like antennae, and the female has a spike, often like a sword, for laying her eggs; in the Grasshoppers the antennae are short, and the female has no spike.)

The Great Green Bush Cricket is in season from the beginning of August until early Autumn. As well as in the colonies and in Southern Europe its penetrating shrill is a familiar sound on warm evenings among the thistles on the Downs, in thickets of rough herbage in sunny corners, on the lee of woods and in the rough, wild ground of most of our southern counties, but it is heard less in the Midlands, and only occasionally in the north. It has been noted from Hexham in Northumberland, but not yet recorded from Scotland or Ireland.

It is not a particularly nimble creature and can be coaxed to leap into the net, but a highly original and much simpler method has been discovered by Herr Feser, of Hamburg, and described in a recent paper by Herr Weidner, of the Hamburg Zoological Museum. Herr Feser took a twig of hazel about 18 inches long and, by tickling the insect gently with it, induced the creature to crawl upon the stick, which must be held upright. The cricket starts to climb until it reaches the top, where it sits quietly, without attempting to fly away. Herr Feser not only carried his stick about for some time with the insect perched on top, but actually caught more. By the end of the day he was carrying no less than seven of them clinging to the stick. When the second one reached the top, he turned the stick round slightly to make room for it and repeated this rotation with each. Another day he succeeded in catching no less than fourteen of them, arranged in rows up the stick, all head-upwards. They seemed perfectly contented so long as the stick was held vertically.

Herr Weidner makes the interesting suggestion that this odd habit was known to the ancients, and quotes a relief in the British Museum from Kuyundjuk, showing figures of servants carrying delicacies to the table of King Asurbanipal. Some are carrying hares, others partridges or francolins, cakes and dessert. Two are bearing a stick in each hand, on each of which stick are a number of creatures definitely recognizable as grasshoppers. They can be distinguished, though not so clearly, on the photograph reproduced here, attached to the two sticks carried by the man on the left. They all have the heads upwards, but the short antennae show that they are not crickets, but true grasshoppers.



It is not easy to see why the locusts, which are actually large, swarming grasshoppers, should be served like this. It does not seem likely that they were pinned in some way to the stick and so submitted to royal inspection. Perhaps they were served, if not alive, at least fresh, so that the king could fry or roast them on a chafing-dish, but it is even more likely that they were roasted and served on a stick, like a Turkish *kebab* or a Caucasian *shashlyk*. Presumably the king treated them as we handle shrimps, tearing off the horny parts and eating the soft body only.

It is well known that in arid countries the inhabitants take revenge upon the locusts that devastate their crops by eating them, and in some countries they are a known commodity in the bazaars. The Desert Locust, *Schistocerca gregaria*, is almost invariably the species in question.

Dr Uvarov informs me that in some parts of Africa a Bush Cricket not very remote from our own Great Green one, by name *Homoeocoryphus vicinus*, sometimes occurs in great numbers and is sold in the bazaars threaded on a string.

Malcolm Burr

A New Plastic

The possibility of the use of lignin is reported by *Scientific American* as an important plastic discovery. The chemical nature of lignin is still uncertain; it is the strengthening material in plant cell walls; it has hitherto been regarded as a useless product of wood waste and pulping mills, and as a hindrance in the manufacture of cellulose products generally. The fact that it is a product from waste gives it a great advantage over other plastics whose usage is handicapped by cost. The new material is expected to be adaptable for use in wall-panelling, electric insulators, wireless cabinets, and table tops, and among its qualities are high electrical resistance and imperviousness to water.

The Ellsworth-Wilkins Expedition

Recently the Norwegian ship, the *Wyatt Earp*, sailed from New York for Capetown, where Sir Hubert Wilkins joins Ellsworth in his Antarctic expedition to Enderby Island. This is Ellsworth's fourth expedition to the Antarctic, and it is his dream to establish a permanent camp at the South Pole. A relay there would mean a triangular link by air between South America, Australia and Japan, as the base would be no farther from Australia than from South America or South Africa. Ellsworth will profit by the experiences of the Russian Expedition whose notes and calculations will be at his disposal, and it is to his great advantage that, contrary to the North Pole, the South Pole is not a drifting iceberg but a continent whose contour is becoming almost as familiar as that of the Australian coast.

The expedition is accompanied by an aircraft carrier, an ice-breaker designed by Ellsworth, and Sir Hubert Wilkins' famous fluted submarine. The exterior

surface of the submarine's shell is fluted and can be heated, so that, when it comes into contact with an iceberg, it can melt its way through. Its watertight bulkheads are three times as thick as those of an ordinary submarine, and although only seven people can be accommodated in it, it can remain submerged for seventy hours; the large, thick window in it should make it possible to take interesting submarine films.

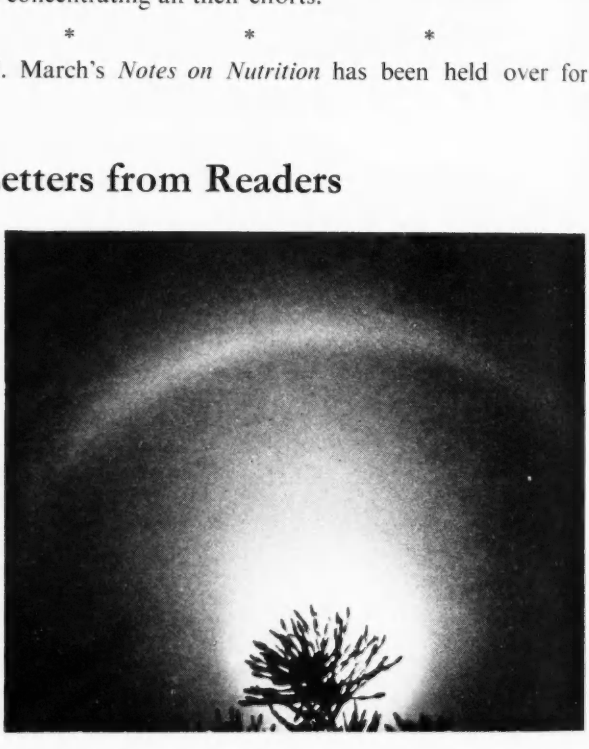
This expedition shows that the United States do not intend to stop at the failure of their North Pole venture, and, since a North base, if it ever does come to be established, would not be of the same importance as a base at the South Pole, it is towards this that they are concentrating all their efforts.

* * * *

The third of Mr C. H. March's *Notes on Nutrition* has been held over for the November issue.

Letters from Readers

A Lunar Halo Photograph



The accompanying photograph of a Lunar Halo is, I believe, unique. The spectacle itself is unusual but, here in Australia, during the last twelve months, the phenomenon has twice appeared, preceeding by a few days the breaking of a long period of drought conditions and bearing out the popular belief that a Lunar Halo is an infallible sign of rain. This halo was of extraordinary brilliance and size—too large to be wholly included in the cameras' field of view—and was remarked by many people over a wide area of the state of New South Wales. It lasted about two hours, from 7 until 9 P.M. when the clouds became thicker and the halo slowly lost its brilliance and faded into darkness.

The photograph was made using a fast film but no screen. An exposure of one minute at f 3·5 was given, the moon itself being hidden behind the branch of a tree to save undue halation. At the time the moon was near the zenith and the planet Jupiter, which can be seen as a white spot to the left of the branch, was in close proximity to the moon. The photograph has not been retouched in any way.

M. S. CHISHOLM.

Goulburn, N.S.W.

Lathraea clandestina

A year or two ago a party of botanists, of which I was one, discovered a very interesting plant in an East Sussex wood. This was *Lathraea clandestina*, which is a parasitic plant native in sub-alpine Europe.

Structurally the species is adapted for a completely parasitic existence; it has no chlorophyll (the green colouring matter to be found in plants with which the essential process of photosynthesis is carried out). Thus it must be entirely dependent on its "host" plant, usually Alder, to which it attaches itself by means of fleshy suckers, for its nourishment and hence its existence.

The occurrence of this curious plant in Sussex is extremely mysterious, and it sets a pretty problem for the specialist in seed dispersal to account for its appearance. Indeed, it is thought that the seeds of parasitic plants will germinate only when in actual contact with a suitable "host" plant. *Lathraea clandestina* is not a species which would be likely to be cultivated, and it seems inconceivable that human agency should have introduced it to so remote a place.

LANCE DAYL

Romsey, Hants

The Dreaming Mind

III. Telepathic Dreams

By R. L. MÉGROZ

THE commonest kind of record of a vision afterwards confirmed by facts is that in which the dreamer has a premonition of death, or it may be a detailed picture of some distant person who is dying or in great danger. So many stories of this kind have found their way into memoirs and other books that everyone must be aware of the type of experience referred to. In order to devote most of the space to records supplied to me by contemporaries I will refer very briefly to earlier records, first reminding the reader that in the most ancient literature a clear distinction has been made between "true" and "false" dreams. Psycho-analysis has shown us that the "false" dreams reveal emotional truth, but we are only on the threshold of a study of knowledge conveyed by a "true" dream. The only kind of "true" dream that has been seriously considered by most scientific people is the dream in which the mind solves a problem that has occupied it in waking hours. That

there is a creative activity possible to the dreaming mind can hardly be disputed any longer. What is disputed, or rather ignored, is the possibility of some kind of extra-sensory perception in the dream. The person who is sceptical about this usually brushes aside the innumerable records as "tall stories" or as due to coincidence, but no responsible critic to-day can apply the word coincidence to the most impressive records of dream revelations.

As old as the idea of "true" dreams is that of the soul travelling away from the body in sleep. It was commoner in the early oriental civilizations than in that of the Egyptians, who stressed rather the sensitiveness of the sleeping mind to mental messages. The ancient Vedic literature of India formulated the theory that the soul roams away from the body in sleep, and enjoined, as do many savage races, that a sleeper should not be suddenly awakened lest the soul may not find its way back to the body.

This idea has taken many forms in different parts of the world. The old Teutonic belief was that the soul issued as a bird from the mouth. A very attractive version is that of the Burmese, who by a derivation from the Buddhist *Manaw*, or sixth sense, evolved a notion of the soul as a kind of fairy—the leyp-lya, or butterfly spirit—which escapes from the mouth at the point of death, and in life is the cause of dreams by roaming about during sleep.

The Egyptians' idea of the mind's hypersensitiveness in sleep suggests a ready explanation of the numerous experiences recorded of people who learn things in dreams which concern them personally. Every century produced its crops of such stories in Europe and it will suffice to start with the nineteenth century. A typical case was sent to the S.P.R. by John Ruskin who questioned Arthur Severn, the painter, and Severn's wife, for exact details. The Severns were in the Lake District, by Coniston. Arthur Severn had gone out early for a sail on the lake, leaving his wife asleep. She woke up suddenly, feeling that she had been struck a blow on the mouth and that her upper lip was bleeding. She held her handkerchief to it and was surprised not to see any blood when she removed the handkerchief. At this point she realized she had been dreaming, noticed her husband was not there, and saw by her watch that the time was 7 o'clock. Severn came in to breakfast at 9.30, holding a handkerchief to his lip, and told his wife that in a squall the tiller of the boat had swung round and cut his upper lip, which would not stop bleeding. She asked him the time of the occurrence, and he said it must have been about 7 o'clock.

Nobody need believe such a story, but if frank disbelief calls for a reason, what reason can be given except the dishonesty or the stupidity of the principals? There are to-day a great many distinguished people who must be charged with dishonesty or stupidity to save the face of the sceptic confronted by scientifically inexplicable experiences.

The late Sir Rider Haggard wrote a letter to *The Times* describing a dream in which he saw his dog lying dead by the side of a railway track. The dog was missing when he awoke, and subsequently its dead body was found at the spot seen by the dreamer.

"I myself had a similar dream", wrote Mr Ralph Durand, another author, to me. "I know that the exact time of its occurrence was after 7 a.m., for I had been called at that hour and had fallen asleep again. The dream was very vague and lacking in detail, but I awoke for the second time with so acute a feeling that my little dog was in great distress that I mentioned it at the breakfast table. The dog was nowhere about but this gave me no concern as it was his habit to leave the house early every morning and range in neighbouring woods till at mid-day he returned for his dinner. At mid-day the dog was still missing. At 3 p.m. I was in my garden and heard him whine. Looking up I saw him standing in the gravel path. I called him but he made only one feeble limp towards me. I went up to

him and found his front paw was caught in a rabbit snare. It was obvious that in his struggle he had wrenched out of the ground the peg that held the snare and had painfully dragged home both wire and peg."

"I am inclined to believe", adds Mr Durand, "that the dog was in the snare at 7 a.m., that in his distress he thought of me and, very likely, tried to call me to come and release him; and that my mind, being in a receptive condition owing to my being asleep, was impressed by the message."

Now the idea of telepathy between an animal and a human being is startling, even if telepathy between human beings is accepted. Moreover, in cases like that of Rider Haggard, the animal was dead, or perhaps dying, when it sent the message, if it did send any thought message. In this or in Mr Durand's dream the animal must be the agent, and the presumption is that it could send out a mental message strong enough to impress a human mind already in sympathetic contact. The extent of our ignorance about the nature of thought, whether in animals or man, is so great that the possibility of such a thing need not be dismissed. One might conjecture that the transference of the message would depend upon the sensitiveness of the recipient's mind rather than on the mental energy of the animal. But there is the further possible conjecture, if one wished to avoid the telepathy hypothesis, that can be based upon the diffused state of the mind in dreams. This diffusion and extension of thought in sleep has often been remarked by psychologists, and it may be only another version of the primitive idea of travelling away from the body by a part of the self that theosophists call "astral".

If certain dream experiences seem to show that our conceptions of space need revision—for clearly this idea of the mind being apart from the body while it perceives what is dreamed can be nothing more than an inadequate image for the reality—Mr J. W. Dunne, and many less systematic recorders of their dreams, have shown that other experiences similarly call into question the nature of time. The problem presented by dreaming the future however had better be considered separately from that of seemingly telepathic dreams, but it is referred to here as a necessary introduction to a dream involving both the telepathic theory and the anticipation of a future event, though the anticipation is not quite in the same order as what may be called the Dunne type of dream that gets in front of time. It is inseparable from the extra-sensory perception, whether by telepathy or by perceiving at a distance, which we have been considering.

Reviews

The Birds of Canada

THE Introduction to this book* states that its objects are (inter alia) to awaken and stimulate an interest both aesthetic and practical in the study of Canadian birds; to suggest its sentimental, scientific and economic value; to assist in the identification of native species; to enable the economist to distinguish bird friend from bird foe, and to present data for protective legislation.

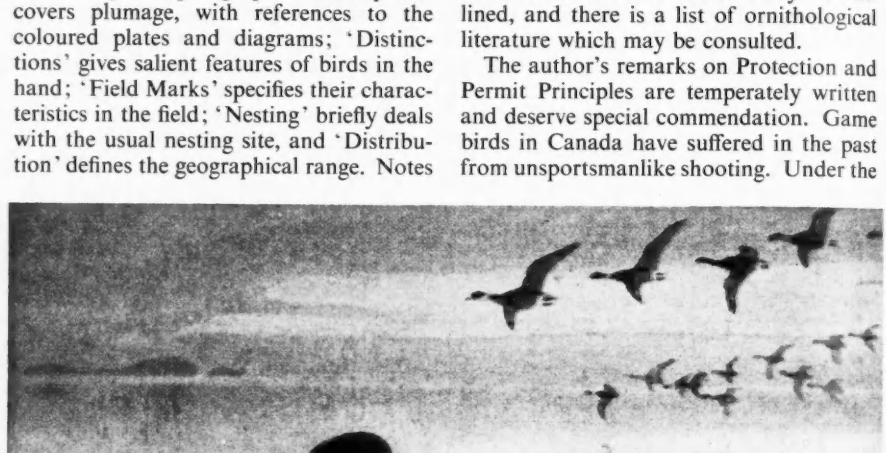
The book, which is published by special permission of the National Museum of Canada, is admirably conceived for the achievement of these objects. It reveals throughout a broad and sane outlook: its information is exhaustive, and it well illustrates the varied appeal of ornithology.

The systematic arrangement employed is that of the American Ornithologists' Union, with certain variations as to vernacular

* *The Birds of Canada*, by P. A. TAVERNER. Murray. 18*s*.

names. The sections allotted to each bird contain separate paragraphs; 'Description' covers plumage, with references to the coloured plates and diagrams; 'Distinctions' gives salient features of birds in the hand; 'Field Marks' specifies their characteristics in the field; 'Nesting' briefly deals with the usual nesting site, and 'Distribution' defines the geographical range. Notes made as to the best ways to attract birds. The various methods of bird-study are outlined, and there is a list of ornithological literature which may be consulted.

The author's remarks on Protection and Permit Principles are temperately written and deserve special commendation. Game birds in Canada have suffered in the past from unsportsmanlike shooting. Under the



on 'Song' are added in many instances. There are also very valuable dissertations on 'Economic Status' in which the evidence for and against the utility of each species is impartially stated.

The Introduction will prove to be of great value both to students of ornithology and to amateurs. Geographical races and variations, systems of nomenclature, and the various theories concerning migration and its origins are explained. Suggestions are

Migratory Birds Convention with the United States, migrant insectivorous birds, and many others, are protected throughout the year in Canada and the United States. Though this enactment has had a beneficial effect, the author expresses anxiety as to the future of waterfowl. The establishment of wild-land reservations which afford sanctuary among cultivation, and the National Parks, are hopeful features, and seem to be the only method by which

much of the original wild life can be preserved.

Permits are granted under the Convention for the sole purpose of scientific study, and under certain conditions designed to prevent waste of bird life, the gross violation of which may be grounds for refusal or revocation of a permit. Such permits need careful watching. The introduction of the European sparrow and starling is deplored, and serves as a warning against similar experiments in our own country.

Canada's vast territory possesses an immense and varied avian fauna. It includes humming birds in British Columbia and snowy owls and ptarmigan from the barren grounds in the far north. The British ornithologist will find described here many birds which are unfamiliar to him except in captivity. We have no tanagers, vireos, juncos, cardinals, cranes, king-birds, pelicans or towhees, and our lists of ducks, warblers, swallows, sparrows, owls, gulls and geese, and many other species, seem meagre when compared with the Canadian varieties.

There is a surface similarity between certain Canadian birds and those of the same species which occur in our own country. For instance, most of the Canadian woodpeckers (and the allied flickers and sapsuckers) have some characteristic which reminds us of one or other of our own three woodpeckers, and the same applies, *mutatis mutandis*, to an even greater extent to the warblers and *paridae*. In contrast to this, Canadian cuckoos show in their calls no resemblance to our own "wandering voice", and in their breeding habits are only occasionally parasitic, a fact that suggests the possible origin of this idiosyncrasy in other cuckoo species. The cowbird—a member of the *icteridae*—is the only Canadian bird which is habitually parasitic.

It is inevitable that in a country where man is encroaching more and more upon lands which until comparatively recent times retained their primeval character, the native fauna should be adversely affected. The author constantly deplores the danger of the extinction of species which are, or are alleged to be, detrimental to farming, fruit-growing and other industries, or to the sportsman or fisherman. Of the risk of a species becoming extinct before its diminution is realised the passenger pigeon is a melancholy example.

The Migratory Birds Convention is a fine achievement, but the need for vigilance by protectionists is constantly emphasised. Loons, those weird and beautiful denizens of the Canadian waterways and lakes, are, owing to the encroachment of settlement, growing scarcer. They are strictly protected at all times, but anglers blame them for the destruction of fish, though the small size of their gullets prohibits their taking any but the smaller kinds. Ducks, in dry weather, are poisoned in hundreds by feeding in alkaline waters, and they and geese are threatened by the disappearance on the Atlantic coast of the eel grass which forms their winter food. Trumpeter swans are decreasing, and strenuous efforts are being made to preserve the last remaining individuals: their position seems to resemble that of the kite in Wales. The Canada Goose, which breeds in the prairies, is particularly susceptible to the evils of encroaching settlement and civilization. Jack Miner's well-known success in domesticating this bird without impairing its wildness and independence is quoted as an example which deserves imitation.

Protective legislation is only effective if backed up by public opinion. This book, packed with information and beautifully illustrated with coloured plates and black and white drawings and diagrams, will be invaluable both to the amateur and the expert; it should do much to stimulate general interest in Canadian avifauna, and bring home the importance of its preservation before it is too late. Canadians possess a magnificent heritage of bird-life: its protection is a duty which they owe not only to themselves but to the whole world.

E. W. HENDY.

Physik

Physik, by Ing. Dr P. WESSEL, Paar. (Verlag Ernst Reinhardt, Munich.)

THIS is intended as a students' text-book. As it is entirely elementary it avoids any algebraical deductions and contains a great number of statements, definitions, and descriptions. It consists of four parts: a treatise; a summary and collection of formulas; over one thousand examination questions; and tables of physical constants. The contents range from the standard classical material to the introduction of neutrons, de Broglie waves, quantum mechanics, group velocity, uncertainty relation, and theory of relativity (where Einstein's name is courageously mentioned). But although these tables contain a collection of the dimensions and units of the more frequently used physical quantities, they fail to include modern interesting alloys, such as permalloy and nicalloy. Nevertheless, the book will clearly be useful to the student who looks upon physics as a branch of science to be dealt with as quickly as possible.

EGON BRETSCHER

Select List of Books Received by Discovery

(*Mention in this list does not preclude review*)

One Hundred Years of Astronomy. R. L. WATFIELD. (Duckworth, 21*s*.)

Starcraft. W. H. BARTON and J. M. JOSEPH. (Mcgraw-Hill Co., 10*s*. 6*d*.)

Practical Seismology and Seismic Prospecting. L. D. LEET. (Appleton, 25*s*.)

Elements of Physics. A. W. SMITH. (Mcgraw-Hill Co., 22*s*. 6*d*.)

On Understanding Physics. W. H. WATSON. (Cambridge University Press, 7*s*. 6*d*.)

British Chemical Industry. SIR GILBERT T. MORGAN and D. D. PRATT. (E. Arnold and Co., 21*s*.)

Scientific Encyclopaedia. VAN NOSTRAND. (Chapman and Hall, 50*s*.)

Dictionary of Scientific Terms. L. M. BEADNELL. (Watts, 5*s*.)

Science for the Citizen. L. HOGBEN. (George Allen and Unwin, 12*s*. 6*d*.)

Physical Science in Modern Life. E. G. RICHARDSON. (English Universities Press, 8*s*. 6*d*.)

Science for the Home. A. M. LOW. (Nelson, 5*s*.)

Scientists are Human. DAVID LINDSEY WATSON. (Watts, 7*s*. 6*d*.)

The Origin of Life. A. I. OPARIN. (Macmillan, 8*s*. 6*d*.)

A Simple Guide to Television. SIDNEY A. MOSELEY and H. J. BARTON CHAPPLE. (Isaac Pitman and Sons, 1*s*.)

Broadcasting from Within. S. W. SMITHERS. (Isaac Pitman and Sons, 2*s*. 6*d*.)

The Chemical Studies of P. J. Macquer. LESLIE J. M. COLEBY. (George Allen and Unwin, 6*s*.)

Quartz Family Minerals. DRAKE, FLEENER, and BEN HUR WILSON. (Whittlesey House, 12*s*. 6*d*.)

Negative Ions. H. S. W. MASSEY. (Cambridge University Press, 6*s*.)

Life Heat and Altitude. DAVID BRUCE DILL. (Harvard, 10*s*. 6*d*.)

Plant Growth Substances. HUGH NICHOL. (Leonard Hill, 3*s*. 6*d*.)

Photography of the Figure. CHARLES SIMPSON. (Witherby, 21*s*.)

Manual for Comparative Anatomy. L. P. SAYLES. (Macmillan and Co., 7*s*. 6*d*.)

Good Citizens. AMABEL WILLIAMS-ELLIS. (Gerald Howe, 3*s*. 6*d*.)

British Calendar Customs (*Vol II, England*). WRIGHT LONES. (William Glaisher, 15*s*.)

Waters of the West. KENNETH PRINGLE. (George Allen and Unwin, 12*s*. 6*d*.)

Living with Lepchas. JOHN MORRIS. (Heinemann, 15*s*.)

OSTRAND.

L. M.

HOGBEN.
6*d*.)

. E. G.
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